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ON THE PROPOSED UNIVERSITY OF THE UNITED STATES AND ITS POSSIBLE RELATIONS TO THE SCIENTIFIC BUREAUS OF THE GOVERNMENT.

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FOR some years the proposition has been discussed that a National University should be established in Washington to represent that projected by Washington himself, and for which he provided, as he supposed, in his will. The foundation, for various reasons, did not materialize, and though one of the reservations in the original plat of the capital city was designated by Washington as a site for the proposed university, no funds being forthcoming, the scheme until lately has remained dormant.

Recently, owing to the interest and enthusiasm of a number of friends of education, the scheme has been revived and much popular interest expressed; several bills have been laid before Congress, and steps taken toward securing popular subscriptions, and the use of a site in the District of Columbia, on one of the public reservations for projected buildings, to be used in connection with the work of the university.[1]

[1] The originally designated plat was afterwards used for the Naval Observatory, and has recently been abandoned, partly on account of the prevalence of malaria.

As far as university instruction of the ordinary American type is concerned, the District of Columbia is already well supplied with the means of furnishing it. It is only necessary to refer to the names of the Georgetown University, the Catholic University, the Columbian University, Howard University, the National University, the proposed American University, and their associated special schools, to make this plain. While nearly all these institutions are more or less distinctly under the control of some religious denomination, I believe none of them confine their educational efforts to students of any one particular faith, and in most of them instruction is sufficiently free from sectarian bias to render the denominational control a matter of little importance to their students, except in so far as it tends to preserve a good standard of morals.

It is, I believe, admitted by the friends of the projected institution that there is no sufficient reason for establishing a new competitor for the opportunity of giving undergraduate instruction. Few friends of liberal education would advise that to the workers in a field already so well occupied, and most of whom are so poorly endowed, should be added another institution of similar character and aims. Generous givers might far better contribute to the strengthening of those already established. But it is claimed, and with some show of reason, that there is still room for an institution of a different character, in which those who have already acquired the essentials of a liberal education could pursue special branches of study, utilizing the opportunities which might be afforded by the government laboratories in various departments of science, to make of themselves highly skilled specialists, for whom the growth of the country is beginning to open a career.

Assuming, for the purposes of argument, that this contention is just, the present paper is intended to discuss, from the point of view of the official man of science, the practical questions of what relations between such an institution and the executive departments of the government are practicable and advisable; and also the organization best suited to promote harmonious and successful coöperation between the departmental laboratories and the members of such a university.

It must be said that the projectors of the enterprise have so far dealt chiefly in generalities, and have hardly touched upon the practical side of the question, which, nevertheless, is a factor upon which the success or failure of the scheme must very largely depend. In fact, most of the documents relating to the proposed university which I have been able to consult absolutely ignore this side of the matter, and even display an apparent ignorance of the conditions which have to be met. It is in the hope of throwing some light upon them, without partiality for or against the project, and in the hope of eliciting further information by discussion from those especially qualified to give it through their connection with the government laboratories, that this paper has been prepared.

Some thirty-four years' experience in the scientific work of the government has given the writer a tolerably good insight into the methods now or formerly in use and the conditions of this side of the problem. During this time the expansion of the scope of this work has been very great, and with the expansion has necessarily come more or less severity of restriction for the purpose of fixing responsibility, controlling expenditures, and defining the limits of work to be authorized. These restrictions have much increased the labor and difficulty of carrying on the work, and to some extent the expense of it. Every worker has realized this, and most have felt disposed to criticise it. The restrictions are frequently double-edged; made by legislators unfamiliar with the methods of science, and having one object in view, they sometimes, whether accomplishing that object or not, bear very severely on the worker in some other direction not at all originally in contemplation. Nevertheless the government has, on the whole, been generous, and the restrictions for the most part beneficial in that, if rigidly lived up to, they protect the scientific bureaus from ignorant and unjust attacks from those with a morbid appetite for scandal. In the last instance, somebody always has to be trusted, and the narrower the field for the exercise of untrammeled judgment, the less subject to unreasonable criticism is the person upon whom the responsibility is laid. This responsibility is divided between the executive head of a bureau and his subordinates

in various measure, but the chief part of it is shared by the Director and the "heads of divisions" who have the direct superintendence of the details of the work. The former, subject to the approval of the Secretary (which in most cases is given as a matter of course), decides the policy of the bureau in its special functions, the allotment of money and work to the different divisions, and the general character and quality of work which shall represent the bureau. He is also the general intermediary between the Department and Congressional committees concerned with the special work of the bureau, explaining the necessity for particular expenditures for which authority is asked, or the propriety of any action about which question has arisen.

The "head of a division" has generally the immediate control of work decided upon and of the special workers, supervises methods and estimates cost, is responsible for accuracy and economy in the use of the fund allotted to the work of his division, and the attendance and efficiency of those engaged in it. Upon him the Director relies for most details, and to him the individual workers look for their instructions.

Both the directors and the "heads of divisions" are usually overworked, and the latter are almost invariably underpaid. The necrology of the scientific staff from year to year shows a lamentable number of early deaths from causes directly or indirectly connected with overwork, "burning the candle at both ends." The temptation of the opportunity for research offered by government laboratories, and unequaled elsewhere, is responsible for the presence in them of many men who in private life would be enjoying the frugal living, high thinking, and long summer vacations of colleges, or from five to ten times their present salaries as consulting experts. Another feature of life in the laboratories is exemplified by the presence there of men who, by years of labor in their specialty, are known around the world as experts of the highest rank, whose contributions to science in a single year far outweigh the best college thesis for the doctorate of philosophy or science, and yet to whom the grant, by some appreciative Faculty, of this modest badge of honor would rouse from the mass of educators a storm

of protest. Such instances could be mentioned, and show conclusively how little general knowledge exists among educated men, not scientists, of the kind and quality of work turned out by the scientific bureaus. This brief statement of conditions is necessary for the clear understanding of the points which are to follow. Addressed to an audience of officials it would be unnecessary.

It is the writer's opinion that the university should be free from the trammels of government control, and that it should ask from Congress only its charter and the privileges of the laboratories; that it should not be a government institution, but should stand on its own merits. Perhaps the grant of a site for the university offices, on one of the larger reservations near the government buildings, might be accepted, as in the case of the Smithsonian; provided it was clearly understood that this did not constitute the university a governmental entity. The objections to its becoming such are many and serious, and will not be enlarged upon here; that it would dry up the springs of private bounty is certain, and is sufficient to condemn the proposition.

On the other hand, the grotesque project of forming its governing board of a dozen active presidents of existing colleges is so preposterous that it only needs to be stated to meet its fate with thinking people.

The university should have for executive purposes a governing board solely its own, and as small as possible, both for efficiency and economy. The faculty should decide on all matters connected with teaching and discipline, and the alumni be granted advisory status on large questions. One or two members of the executive board should be taken from the list of directors of scientific bureaus, but in their private, not their official capacity. A board wholly inexperienced in government routine and conditions would be constantly in hot water.

Various branches of university training stand somewhat outside the laboratory work, though more or less dependent upon the libraries and archives of the city. For these the appointment of professors would be required. There should probably

be a few administrative members of the faculty covering the branches which did avail themselves of the laboratories, but more to act in an advisory capacity to the student than to teach him. The laboratory student while at work should know but one executive head, the chief of division in whose laboratory his work is done. Any division of authority here would be fatal.

We would have then a small executive board, a small faculty, and, as it naturally follows, small administrative expenses. Concentration of power in the hands of competent men is the soul of efficiency and the warranty of success.

No funds should be sunk in pretentious buildings. A single building, with one large acoustically perfect hall, and as many smaller lecture rooms as seemed requisite, with offices for the archives, bursar, and administrative men, would be all that would be really necessary or useful, at all events for some time to come. Under these circumstances the funds contributed could be almost wholly devoted to the true purpose of such a university, the production of highly trained experts, and the endowment of research.

It is obvious that the interests of the government's own work would permit of only a small number of students in any one laboratory, such a number, in each case, as the chief of division felt certain could be advantageously utilized and controlled. It would be impracticable to admit professors or classes into any laboratory except as rare visitors, such as occasionally come now.

The laboratory student must come, if at all, as the regularly employed workers come, to keep the same hours, observe the same rules, and render to the chief the same obedience. For the class of men we are considering as possible students this would not be a grievous requirement. The method of instruction would necessarily be that of Agassiz. Actual work on actual material, with results in sight from the first, and methods absorbed through contact and experience not merely experimental. I think there are few chiefs of division who would not welcome one or two well-trained enthusiastic students under such conditions.

The question then arises as to how the reception of students might be controlled and organized. A simple resolution or bill in Congress, authorizing the scientific bureaus to admit, subject to the approval of the director and chief of division, such students as they may find qualified, and who can be employed with advantage to the work of the bureau, would be all the legislation that is needed; unless Congress should require that their presence should involve the government in no expense or responsibility, and authorize the officers above mentioned to make rules to cover the conditions. As these are different in each laboratory, the rules should be left to the authorities of each laboratory. The function of the university, as such, in the case of these students would then be limited, as in the case of London University, to a determination of their qualifications and the issuing of an equivalent degree, not necessarily by oral examination, but on the record of work accomplished, if it proved desirable. For such men the acquisition of the qualifications should regulate the duration of study, not some arbitrary period of time. The Director of the bureau should be authorized to accept or reject students, because he is responsible for the work of the bureau, and the Chief of division because upon him falls the responsibility for the success and proper conduct of his own divisional work, and whatever labor and time is required to direct and utilize the student. The position of the Chief of division with relation to the student and the university will then be that of a tutor or docent, and in return for his services to the student the university should provide a modest honorarium which might be refunded to the university by the student, or deducted from the amount of a scholarship if the student held one, or paid by the university as endowment of research. It would be better that no private arrangement between teacher and pupil should be permitted, but that such transactions should be handled by the university authorities, for obvious reasons. If the university were a government institution, it could not pay fees to any government official under the present law, which is not likely to be changed. It would be obviously unjust to add to the regular official duties of a laboratory chief the responsibility involved in the recep-

tion and supervision of pupils, without some remuneration. For the infinitesimal cost which might indirectly fall upon the United States through the presence in the laboratory of one or two students, the government would be amply repaid, both by the gratuitous labor of the student and by the creation of a body of experts already trained to government methods who might be available for sudden emergencies.

There remains to be provided for, the method of selecting from among candidates those who should be admitted to the privileges of the laboratories.

Candidates might be required to present to the proper officer of the university certificates of graduation, proficiency, experience, and moral character, with a statement of the line of work they desired to take up. These having been classified, the directors of the bureaus concerned, on notification, might appoint the chiefs of those divisions for whose privileges application had been made, and who should meet as a board or committee to discuss applications and report their decisions to the various directors. The conclusions of the committee having been ratified by the directors, and referred back to the board, could by it be transmitted to the university authorities, who could then announce to the successful candidates that, on matriculation and payment of university fees, they would be duly accredited to the laboratories concerned.

This method would enable the university annually to allot a small but picked body of the most promising students of the country to those places where they could get unique opportunities for special work; and would, in the course of time, produce a body of experts, many of whom would naturally gravitate into the government service, and all of whom would be available for special services to the government, if needed, in a way no other method of training could supply. If the university graduated only twenty such men in a year, it would more than justify its existence. It would thus not compete with any other institution, and would supply a training and experience not to be gained elsewhere.

There are of course, as in all human affairs, opportunities for friction and criticism in the plan proposed. The general

proposition that such a body as the proposed university should be admitted to such privileges is one upon which differences of opinion might naturally exist, and which is not discussed in this paper. Here I have assumed the affirmative reply to the general question, and merely presented for criticism and discussion the outline of a comparatively simple scheme by which the proposed relation between such a university and the government laboratories might be carried into effect. That it is practicable I am convinced from the experience of former days, when the towers of the Smithsonian sheltered a body of mostly impecunious but enthusiastic volunteer students, under the supervision of Henry and Baird, almost every one of whom in later days became distinguished for services rendered to science.

Should the plan suggested fail to recommend itself to the promoters of the new university, it would still be possible for any existing institution of learning, or any number of them in association, to avail themselves of the undoubted opportunities herein pointed out. The formulation of plans to this end would be simple and easy. In this connection I may quote a few paragraphs from an abstract of the current annual report of the Secretary of Agriculture, which has appeared in the daily press since the preceding paper was written.

THE DEPARTMENT AS AN AID TO POST-GRADUATE WORK.[1]

Regarding the facilities of the department for post-graduate instruction, the secretary says there is no university in the land where the young farmer may pursue post-graduate studies in all the sciences relating to production, but that the scientific divisions of the Department of Agriculture can to some extent provide post-graduate facilities.

The chiefs of divisions are very proficient in their lines, the apparatus the best obtainable, the libraries the most complete of any in the country, and the studies of a few bright people could be directed in each division, so that when the department requires help, as it often does, the services of these young scientists would be available.

These students should be graduates of agricultural colleges, and should come to the department through an examination that would bring the best young men. The capacity of the department is limited; but assistants are

[1] *Washington Evening Star*, Dec. 2, 1898.

often tempted to accept higher salaries in state institutions, and the opening of the laboratories to post-graduate work would provide an eligible list to fill vacancies as they occur, supply temporary agents, and be a source from which state institutions might get assistance in scientific lines.

ADDENDUM. — The preceding paper, for the purpose of eliciting discussion and suggestions, was read at a meeting of the Philosophical Society of Washington, Dec. 10, 1898, and some of the points raised may advantageously be noted here.

The discussion took a turn toward the distinct proposition of a governmental university, which was considered by Prof. Lester F. Ward and Surgeon-General Sternberg, but which the present writer regards as impracticable, even if desirable, under present conditions.

The points bearing on the proposition advanced in this paper, and which it seems desirable to notice, are as follows:

1. That the organization proposed would not constitute "a university."

The writer is entirely indifferent as to the title of the proposed institution. What he has tried to show is a practicable means of utilizing certain at present unused opportunities of great value to special students.

2. That the plan would not accommodate all who might apply, and that some bureaus might not be willing to accommodate any students.

This is, of course, the essence of the problem. It would in any event be impracticable and unwise to hamper the bureaus by undesired additions to their corps. But the competition for the opportunities would make them even more desirable to the ambitious student, and secure for them the most promising men. The plan is essentially intended as selective of, and only of, the very best.

3. That while in certain lines there might be opportunities for a fair number of students, the fact that there were other lines in which no students could be accommodated would render the distribution of the men among the different branches of science unequal, or, to use the phrase of one of the critics, the "university would be lopsided."

I have never heard of any university in which the number of

students pursuing special post-graduate courses was equal, or nearly equal, in the different specialties. Tastes are not equally represented in the graduate population any more than opportunities in the world at large. At any rate, no more could be utilized than exist, and if the numbers in different lines are unequal, this is no reason why any of them should be wasted.

Doubts as to the workability of the scheme here proposed were only expressed by one or two persons, none of whom had had practical experience in the laboratory work, and I would repeat that I have entire confidence in its practicability, knowing from my own experience that many students have passed from temporary post-graduate employment in the laboratories to lucrative and successful employment elsewhere.

THE RELATION BETWEEN FORESTRY AND GEOLOGY IN NEW JERSEY.

ARTHUR HOLLICK.

II. Historical Development of the Flora.

Within the boundaries of the state are geological formations representing all the great time divisions — Eozoic, Palæozoic, Mesozoic, and Neozoic — and rocks of all the included geologic periods, with the exception of the Carboniferous and Jurassic.

In tracing the development of plant life through geologic time the fact is well recognized that the flora of Eozoic and Palæozoic times is not related to our living flora by any closer ties than those of sub-kingdoms or classes. In Mesozoic time generic relationships may be traced, while in Neozoic time many species either identical with or closely related to living ones may be recognized.

It has also been accepted as a broad generalization that biologic development has been coincident with geologic sequence, or, in other words, that the farther back in geologic time we begin our investigations the lower in the scale of life we find the plants to be; and, conversely, that the nearer we approach modern time the higher they are in development. Plants have developed in the past in accordance with changes in their environments, as they do to-day, so that in order to understand the evolution of any living flora it is necessary to know something about the changes which have preceded the existing conditions.

For the purposes of this discussion we need not begin any farther back in geologic time than the Triassic period, when the shore line of the North American continent, so far as New Jersey is concerned, extended irregularly from about the vicinity of Mahwah to a few miles south of Phillipsburg. This was evidently a period of slow subsidence, and the Triassic deposits were largely laid down in shallow estuaries or lagoons, which

were alternately covered with the tides and exposed to the atmosphere. The rocks are mostly conglomerates, sandstones, and shales, evidently shore or shallow water deposits, often ripple-marked or sun-cracked, and occasionally bearing the footprints of land animals or amphibians which wandered over them.

The vegetation of the period is but sparsely represented in the collections which have been made in New Jersey, but these probably fairly represent its general characters. Dr. J. S. Newberry has described about ten species from the state,[1] of which three are pteridophytes, and the remainder probably all referable to the gymnosperms. One living genus (*Equisetum*) is recognized.

Thus far, in any collection of Triassic plants which has been made, nothing higher in development than the monocotyledons is even indicated, and we may regard the Triassic flora as one composed almost wholly of ferns, cycads, and conifers, with cycads as the dominant type.

Towards the close of the Triassic period great physical changes occurred, of which the extrusion of trap dikes was one of the most prominent features. The indications also are that that portion of the continent now represented by New Jersey and vicinity was raised above its former level and remained so for a long time, while farther south it was depressed, as in this state we know of no deposits which can be even provisionally referred to the next succeeding period, the Jurassic, which, however, occur in Maryland and southward. In New Jersey, therefore, we have a break at this period in the geologic sequence, and in consequence a hiatus in the line of plant development which has been at least partially bridged by Prof. Wm. M. Fontaine and Dr. Lester F. Ward in their studies of the Potomac flora of Maryland and Virginia.[2] The exact geologic age of the lower strata of this formation has not been definitely settled,

[1] Fossil Fishes and Fossil Plants of the Triassic Rocks of New Jersey and the Connecticut Valley, *Monographs of the United States Geological Survey*, vol. xiv.

[2] Fontaine, Wm. M. The Potomac or Younger Mesozoic Flora, *Monographs of the United States Geological Survey*, vol. xv, pts. i and ii.

Ward, L. F. The Potomac Formation, *Fifteenth Annual Report of the United States Geological Survey*, pp. 307–397.

but all the evidence thus far adduced from the fossil plants indicates a transition from the Triassic flora below to the typical Cretaceous flora above.

In this transition flora, accompanying the pteridophytes and gymnosperms, are numerous archaic types of angiosperms and others in which generic relationships with living plants are more or less definitely indicated. Others more closely related are described under such names as *Ficophyllum*, *Sapindopsis*, *Saliciphyllum*, *Quercophyllum*, *Eucalyptophyllum*, etc., while not a few living genera are recognized (*Torreya*, *Sequoia*, *Araucaria*, *Taxodium*, *Sassafras*, *Myrica*, etc.). The number of pteridophytes and gymnosperms as compared with the angiosperms is about 4 to 1, so that the lower types of vegetation were evidently yet in the ascendant.

The limited number of modern elements contained in this and the preceding flora render a comparison with our living flora somewhat hazardous so far as any conclusions as to climate are concerned, but we may safely say that in their general character they indicate tropical or subtropical conditions.

The strata next succeeding the Triassic in New Jersey consist of clays, sands, and gravels, which are apparently Middle Cretaceous in age. This indicates a later submergence of the New Jersey area, when the shore line was approximately where we now find the southern edge of the Triassic outcrop to be, extending from Woodbridge to Trenton.

This was evidently a period of quietude and slow subsidence, as the deposits are largely clays and fine sands in which immense quantities of land vegetation are entombed, many of the specimens being so delicate that it is difficult to understand how they could have been preserved at all, except in very quiet or exceedingly sluggish waters. The occurrence of a few marine molluscs indicates that the waters were at times subject to tidal influence, but essentially they must have been fresh or perhaps brackish.

This flora has been described by Dr. Newberry,[1] who recognized in it 156 species, of which all but about thirty are angio-

[1] The Flora of the Amboy Clays, *Monographs of the United States Geological Survey*, vol. xxvi.

sperms, nearly all of them included under living genera. Many of these now inhabit the region, such as *Diospyros*, *Juglans*, *Liriodendron*, *Magnolia*, *Populus*, *Salix*, etc. ; but others are of more southern distribution, such as *Bauhinia*, *Cinnamomum*, *Eucalyptus*, *Ficus*, *Laurus*, *Passiflora*, *Sequoia*, etc.

No living species is recognized, although close specific relationship is commented upon in several instances, and is indicated in at least one of the names adopted (*Magnolia glaucoides*).

The most significant feature of the flora as a whole is the complete reversal of the proportions between the angiosperms and gymnosperms as compared with their proportions in the preceding flora, the angiosperms being now overwhelmingly in the ascendant, while in the gymnosperms the conifers are more abundant than the cycads.

The genera also indicate a less tropical climate than that which previously prevailed, but one which was considerably warmer than now obtains in the region.

After the clays had been laid down as estuary or brackish water deposits, the submergence continued, and we next find the clay marls, representing the transition to marine conditions. In these the land vegetation is less abundant, but is not noticeably different in its general characters from that which preceded it.[1]

The subsidence continued and true marine conditions supervened. The marls were deposited, and in them nothing but marine organisms are preserved. Thus far we have not found any record of the land vegetation which occupied the region during this period, but in the west the conditions were different, and the remains of Upper Cretaceous plants are abundantly preserved in the Laramie and allied deposits. In these most of the Middle Cretaceous genera are found to continue, and a number of new ones to appear, but the species in all but a very few instances are different, and the monocotyledons begin to assume prominence for the first time, in the form of fan palms.

Generically this flora is more closely related to our living flora than was that which had preceded it. The number of

[1] Hollick, Arthur. The Cretaceous Clay Marl Exposure at Cliffwood, N. J., *Trans. N. Y. Acad. Sci.*, vol. xvi (1897), pp. 124–136.

living genera included in it was actually and relatively greater, and the species are of a more modern aspect; but none is apparently identical with any now living.

The ratios between the pteridophytes, gymnosperms, and angiosperms were approximately about as we find them to be at the present time, and the climatic conditions were apparently yet subtropical.

During the early and middle parts of the next succeeding period, the Tertiary, the indications are that while there were minor oscillations of level, the previous gradual subsidence continued until the shore line had advanced far inland, covering the entire region which we know as the coastal plain, and causing the sediments to be deposited which we recognize in the aggregate as the Yellow Gravel formation. In places this is undoubtedly of marine origin, while in others it is apparently due to floods of fresh water. At one locality only, in the vicinity of Bridgeton, has the flora of this period been found in the state. Fortunately the remains there preserved were collected in abundance and in excellent condition. Probably about fifty well-defined species are represented in the collections which have been made; all of them angiosperms; many of them referable to living species, or so closely identical that it is not possible to separate them; some of the latter the same as species now growing in the vicinity of Bridgeton (*Ilex opaca*, *Nyssa aquatica*, etc.).

A comparison between this fossil flora and the living flora of eastern North America indicates a close identity between the former and that now in existence somewhat farther south, say at about the latitude of Virginia.[1]

Theoretically this Bridgeton flora should be Pleiocene or late

[1] The study of this flora has not yet been completed, but the preliminary conclusions may be found in the following papers:

Palæobotany of the Yellow Gravel at Bridgeton, N. J. Arthur Hollick. *Bull. Torrey Bot. Club*, vol. xix (1892), pp. 330–333.

New Species of Leguminous Pods from the Yellow Gravel at Bridgeton, N. J. *Ibid.*, vol. xxiii (1896), pp. 46–49.

A New Fossil Monocotyledon from the Yellow Gravel at Bridgeton, N. J. *Ibid.*, vol. xxiv (1897), pp. 329–331.

In the above papers may also be found references to the work of others in the same locality.

Miocene in age, but in many of its elements it is unique, and is distinct from that of any other American Tertiary locality. The collections of Eocene and Miocene plants which have been made in the west contain different species, and those from Bridgeton are rare or else entirely wanting in them. As a whole, however, the flora seems to be more nearly comparable with that of certain European Miocene localities, and this idea is also in accordance with the well-recognized fact that plant development was more advanced in Europe than in America. Thus European Eocene plants are in part represented by Miocene plants in America. European Miocene by American Pliocene, and European Pliocene by our present living flora. From its general character I am inclined to consider it as more recent in age than that of any other recognized Tertiary horizon in America.

Towards the close of the Tertiary period an era of elevation began which raised the northern part of the North American continent many hundreds of feet above its former level and extended the shore line out far beyond its former or present position, so that the edge of the continent was about where we now find the one hundred fathom contour to be.

Up to this time in the world's history we have every reason to believe that there were no extremes of climate between the poles and the equator such as prevail to-day. The temperature of the entire earth's surface was more or less uniform during each of the several periods, up to and including the Tertiary, although a constant change had been in progress from tropical to temperate conditions.

The elevation which began in the Tertiary period, however, caused, or at least was coincident with, the greatest changes, climatic and biologic, which are anywhere recorded in geologic history. The climate gradually became more and more severe, and finally culminated in what we call the Glacial epoch of the Quaternary period.

That the changes wrought were gradual, extending over a long period of time, we are justified in concluding, for the reason that the vegetation which was in existence at the time when it was finally overwhelmed by the accumulations of ice

and snow was identical in all respects with that of to-day over the same region. In other words, the flora of the Tertiary period had become modified to the new conditions before its final extermination by the ice sheet, which extended southward in New Jersey as far as Perth Amboy in the east and Belvidere in the west. Every species thus far discovered in the Quaternary clays and gravels, or in old peat bogs beneath the bowlder till, is identical with some living species, and this evidence of modification to meet changing conditions implies a long period of time. Such species as were located within the area of glaciation were of course absolutely exterminated, while others were driven southward, and only such as could exist under these vicissitudes remained to reëstablish themselves after the final recession of the ice.

I do not know of any remains of the vegetation of this period having been found in New Jersey, and such as have been found elsewhere are scanty in amount.

The final recession of the ice was accompanied by a subsidence of the land, and this subsidence was probably the cause of the recession in the same way that the previous elevation had been the cause of its accumulation. Several oscillations of level occurred, and finally the land assumed the contour and topography of to-day.

At the present time, so far as New Jersey is concerned, a slow subsidence of the land is recognized as taking place, which amounts to about two feet per century. This rate of movement, while very slow, is probably no greater than that which produced such tremendous changes of level and such far-reaching effects in the past, and we have but to consider the cumulative effects in order to appreciate that a few centuries hence great changes in topography may be effected. Even within historic times the subsidence of the land has caused the coast line to advance inland in many localities, so that what was once upland has become salt meadow, while salt meadow turf and tree stumps are found far out in the ocean bottom, beyond the present shore line.

Recognizing these facts, the question naturally arises as to the ultimate result, provided the present conditions continue.

Manifestly the flora which occupies the coast region will have its habitat more and more restricted in area, and will be driven more and more towards the tension zone, where the struggle for existence will become fiercer and the weaker elements will succumb. Thus not only are the physical changes in the environment inimical, but also the trend of biologic evolution. The sequence of events in the evolution of the vegetable kingdom show conclusively that the gymnosperm type is a waning one, and that the more highly developed angiosperms have been slowly but inevitably crowding it out since early Cretaceous times, and at the present time, in any competition for the occupancy of a region at all favorable for the angiosperms, these latter are sure to prevail, and the conclusion appears to be inevitable that the flora of the coniferous zone is destined to be ultimately obliterated or only to exist over limited areas, often for the negative reason that in such areas the conditions may not be favorable for the growth of other types. The influence of man may produce temporary changes and give temporary advantage one way or the other, as may often be seen in the occupation by cedars or pines of land which has been recently cleared of deciduous trees ; but such changes are artificial and sporadic and cannot prevail over the constant and inevitable progress of physical and organic evolution.

Not only is the gradual extinction of the gymnosperm type thus indicated, but by the same method of reasoning the angiosperms characteristic of the coniferous zone must of necessity be the first to die out in that class, not only because of the gradual restriction of the area which they occupy, but also because, as is well known, the genera represented are older and the flora as a whole is less modern in its characteristics than obtain in the angiosperm flora of the deciduous zone. The genera of the coniferous zone are largely confined to America, whereas those of the deciduous zone are largely common to both America and Europe. These latter are thus of wide and varied distribution ; they occupy a region practically unrestricted in area, and represent more recently evolved types of vegetation.

THE WINGS OF INSECTS.

J. H. COMSTOCK AND J. G. NEEDHAM.

CHAPTER IV (*continued*).

The Specialization of Wings by Addition.

IV. THE VENATION OF THE WINGS OF EPHEMERIDA.

THE determination of the homologies of the wing-veins of May-flies appears, at first sight, to be an extremely difficult problem; for the wings of these insects are very different from those of any other order. But, as soon as one understands the ways in which the wings have been modified, it is easy to identify the principal veins.

In this order a marked cephalization of the flight function has taken place, which has resulted in a great reduction of the hind wings of all living forms. In some cases (*Cænis et al.*) this has gone so far that the hind wings are wanting.

In a few genera (*Oligoneura et al.*) both pairs of wings are furnished with but few veins. It requires only a little study, however, to convince one that these genera with few-veined wings are degraded and not generalized. It is in the fore wings of those forms in which many wing-veins have been retained that the homologies of the wing-veins are most easily determined.

Fig. 69 represents the venation of a species which will serve well as a type of the recent May-flies; and the lettering of the figure indicates our conclusions regarding the homologies of the veins. But the most characteristic feature of the wings is not shown in the figure. If the reader will examine one of the larger May-flies, he will see that the corrugation of the wings is much more perfect than in any other order of insects, extending to all parts of the wings.

This fan-like structure of the ephemerid wings has been referred to by many writers. But it is worth while to point

out in this place the degree of perfection that has been reached in the alternation of convex and concave veins. In the accompanying table the names of the convex veins, those veins that follow the crests of ridges, are printed in Italics; while the names of concave veins, those veins that follow the furrows, are printed in Roman type.

TABLE OF WING-VEINS OF EPHEMERIDA.

C.	*Costa*			*C.*
Sc.	Subcosta			Sc.
R.	*Radius*			R_1
Rs.	Radial sector	Vein R_{2+3}	Vein R_2	R_2
			Vein R_3	R_3
		Chief accessory radial vein		1
		Vein R_{4+5}	*Vein* R_4	R_4
			Vein R_5	R_5
M.	*Media*	*Vein* M_{1+2}	*Vein* M_1	M_1
			Vein M_2	M_2
		Vein M_3		M_3
Cu.	Cubitus	Vein Cu_1		Cu_1
		Chief accessory cubital vein		*1*
		Vein Cu_2		Cu_2
1st A.	*1st Anal vein*			*1st A.*
2d A.	2d Anal vein			2d A.
3d A.	*3d Anal vein*			*3d A.*

One of the most characteristic features in the venation of the wings of May-flies is that the radial sector plays the part of a principal vein; it originates near the base of the wing; and, as a rule, it is detached, in the adult, from the main stem of the radius.[1] For this reason it is given the position of a principal vein in the table.

If this modification be made, it will be seen that, when the principal veins are considered, there is a strict alternation of convex and concave veins; and that in the case of the forked veins (the radial sector, the media, and the cubitus) the principal branches of a vein are of the same nature as the main stem.

It will also be seen that this alternation of convex and concave veins exists in the distal portion of the wing. In those

[1] In certain Plecoptera and Trichoptera the radial sector of the hind wings is detached in a similar manner.

cases where a vein has an even number of branches (the radial sector and the cubitus) the alternation has been attained by the development of an accessory vein. These are indicated in the table as chief accessory veins, and are lettered 1 in the figure. Many other accessory veins are developed at the margin of the wing in a more or less irregular manner; but whenever a second accessory vein extends far into the disk of the wing it is accompanied by a third, one being convex, the other concave. The anal area of the wing, where the accessory veins are more of the nature of braces, like cross-veins, is not included in this statement, nor in that which follows.

Correlated with the development of a triangular form of wing, which involves an expanding of its outer margin, is the fact that the accessory longitudinal veins are all added distally in the May-flies. But the method of development of these veins appears to be radically different from what it is in the Neuroptera.[1] There the accessory longitudinal veins are preceded by tracheæ, which arise as fine twigs at the tips of older tracheæ, and which in the course of phylogenetic development branch off from the parent tracheæ farther and farther from the margin of the wing, thus making room for the development of other twigs. Here, in the May-flies, the accessory longitudinal veins are evidently thickened folds, which arise more or less nearly midway between other veins. A similar thickening of a fold occurs in the Diptera, where, in certain Asilidæ, the anal furrow is vein-like in structure.

A fact of prime importance in the study of the homologies of the wing-veins of May-flies is that the corrugations of the wing are the most persistent features of it. Hence the most important criterion for determining the homology of a vein is whether it is a concave or a convex one. The basal connections of the veins are very inconstant, and are often misleading. We have already referred to the separation of the radial sector from the main stem of the radius in the adult (its true origin is easily seen when the tracheation of the wings of certain nymphs is studied); and other separations and secondary attachments are common. A good illustration is furnished by the wings

[1] See *American Naturalist*, vol. xxxii, pp. 771, 772.

represented by Fig. 69. In the hind wing, vein Cu_2 is apparently a branch of the first anal vein (marked A in the figure); but in the fore wing, which is less modified, its primitive connection is preserved; although even here a prominent bend has brought it near to the anal vein, and only a step more would be required, the fading out of the basal section, to reach

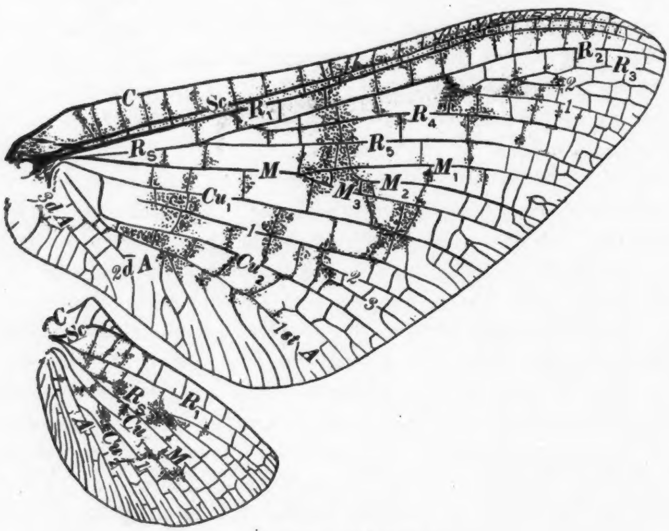

FIG. 69. — Wings of *Ephemera*.

the condition attained in the hind wing. But the concave nature of this vein in the hind wing indicates its homology in spite of its misleading basal connection.

It should be remembered that the convex or concave nature of a vein is the result of a corrugation of the wing and not the cause of this corrugation. The theory of Adolph that the two sets of veins have a different ontogenetic development has absolutely no foundation in fact, as will be seen when we come to study the development of wing-veins, and as was suspected by Brauer and Redtenbacher.[1]

The primitive insect wing was doubtless flat. It makes no

[1] *Zoologischer Anzeiger*, 1888, p. 443.

difference, so far as this point is concerned, whether we believe that the wing is a modified tracheal gill or a transformed parachute-like expansion of the body wall. In either case it is highly improbable that it was fanlike at first. It was not until the wing became an organ of flight that a corrugation of it was beneficial; and even then this corrugation did not spring into existence suddenly, only to be lost in most of the orders of insects; as must be inferred, if we accept the theory of Adolph, that the wing of a May-fly represents the primitive type of this organ.

The stiffening of the costal margin of the wing by the formation of a subcostal furrow has been attained in most of the orders of insects; and in several of them the formation of folds has extended, to a greater or less degree, to other parts of the wing. But, as a rule, this method of specialization has not been the most important one in perfecting the wing. In the Odonata it has been carried farther than elsewhere, among living insects, except in the Ephemerida. But in the Odonata it has been supplemented by other methods of specialization, already discussed, with the result that an exceedingly efficient organ of flight has been developed in that order; while in the Ephemerids the cephalization of the flight function and the corrugating of the wings have been the chief lines along which specialization has extended. The former has doubtless added much to the efficiency of the wings; but a too close adherence to the latter method of specialization has resulted in the formation of a rather indifferent organ; although it is the most perfect development of its peculiar type.

We have studied the tracheation of many nymphs of May-flies, but with results much less satisfactory than those we have reached in the study of other orders of insects with many-veined wings. In all nymphs of May-flies that we have examined, a greater or less reduction of the tracheæ appears to have taken place; and in many of them a large proportion of the longitudinal veins contain no tracheæ. And, too, the presence or absence of a trachea in a vein appears to have little significance. As an example of this the wings of two nymphs are before the writer, in which the venation is so similar that there

is not the slightest difficulty in tracing the homologies of the veins. In one the radial sector and the media contain well-preserved tracheæ; in the other there is not the slightest trace of a trachea in these veins. On the other hand, in the latter the cubital trachea is forked, one of the branches traversing vein Cu_2; while in the former the cubital trachea is simple, there being not the slightest indication of a trachea in vein Cu_2.

The basal connections of the trachea of the wing are very different from what we have seen elsewhere. In the Plecoptera there are two distinct groups of tracheæ which enter the wing;[1] the same is true of certain cockroaches;[2] in all other forms

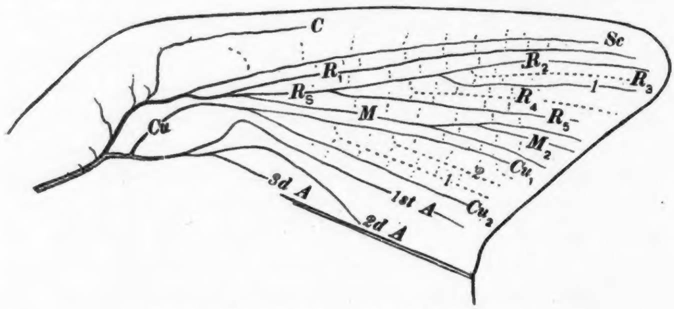

FIG. 70. — The tracheation of a wing of a May-fly nymph.

that we have studied, except the May-flies, a transverse basal trachea connects these two groups, and from this transverse trachea (transverse in relation to the wing, but longitudinal in relation to the body) the principal tracheæ of the wing extend more or less nearly at right angles to it.[3] In the May-flies a single trachea arises from the principal longitudinal trachea of one side of the thorax, and, after giving off a branch to the corresponding leg, passes directly to the base of the wing. Here it divides into several branches which continue in approximately the same direction and become the principal tracheæ of the wing.

In some cases this trachea extends into the wing before it divides. But in other forms, which we regard as more general-

[1] *American Naturalist*, vol. xxxii, p. 238, Fig. 8; p. 239, Fig. 9.
[2] *Loc. cit.*, p. 773, Fig. 56. [3] *Loc. cit.*, p. 772, Fig. 54.

ized, it separates into two trunks in the thorax near the base of the wing (Fig. 70); from one of these arises the costo-radial group of tracheæ, and from the other the cubito-anal group.

Fig. 70 will serve to illustrate what may be considered the type of tracheation of the wings in this order. It was made from a study of the nymphs referred to above. The positions of those longitudinal veins that contained no tracheæ in these nymphs are indicated by dotted lines.

The discussion of the venation of the wings of Ephemerida brings up the question of the venation of the primitive insect wing. For, in several of the more important papers on the homologies of wing-veins, it has been assumed that the wings of May-flies resemble closely the wings of the primitive winged insect.

The great preponderance of the many-veined type among the insect wings that have been found in the Carboniferous rocks has doubtless strengthened the quite generally accepted view that the primitive winged insect had many wing-veins. Thus Redtenbacher states:[1]

> The geologically older Orthoptera and Neuroptera show a much richer venation than the Coleoptera, Lepidoptera, Hymenoptera, and Diptera; likewise among the Rhyncota, the oldest forms, the Cicadas and the Fulgoridæ, possess much more numerous veins than the Hemiptera. There is apparently, then, no doubt that the oldest insect forms were provided, to a certain extent, with a superfluity of veins, and that, in the course of development, all the superfluous veins disappeared by reduction, and in this way a simple system of venation was brought about.

But we have shown that all the existing types of insect wings can be derived from one in which there are but few wing-veins — our hypothetical type, already figured several times. The deviations from this type in the more generalized members of the greater number of the orders of insects is slight. And we have pointed out the ways in which it is being modified, on the one hand by the coalescence of veins, and on the other by the development of accessory veins. While this is easy to understand, it is very difficult to conceive how the wings of the Lepidoptera, Diptera, and Hymenoptera could have been

[1] *Annalen des k. k. nat. Hofmuseums*, Bd. i, p. 153.

evolved from a wing of either the ephemerid or neuropterous type. After a wing had been strengthened by many cross-veins, it is not probable that these should disappear with the exception of the few to which we have applied names[1] in so many different orders, in so nearly an identical manner. Forms with reduced venation occur in most of the orders, but the results of these independent reductions differ greatly from each other. It is necessary, therefore, to examine again the paleontological evidence.

The great preponderance of many-veined wings in the Carboniferous rocks is probably due to the fact that doubtless then, as now, insects with many wing-veins were the ones that lived near water, and were, therefore, the ones most likely to be preserved as fossils.

Another point which should be taken into account is that, notwithstanding the great antiquity of the Carboniferous times, it was a comparatively late period in the history of insects, for winged insects appeared in the Silurian. We are carrying our investigations back only a step, although it is a long one, towards the period when wings were first developed by studying Carboniferous fossils.

Unfortunately, our knowledge of Silurian insects is meager. Moberg has figured an insect from the upper part of the lower Silurian; and Brongniart has figured and described a wing from the middle Silurian sandstone of Calvados, France. This we believe is all that is known regarding the insect fauna of the Silurian; and when we take into account the immensity of the period of time occupied by the deposition of the Silurian rocks, we are forced to admit that we know almost nothing regarding the older insects.

Of the Devonian insects, the remains of several are known. Those which are best preserved are *Homothetus fossilis* (Fig. 71), *Xenoneura antiquorum* (Fig. 72), and *Platephemera antiqua* (Fig. 73). (The figures given here are reproduced from Plate VII of Mr. Scudder's *Pretertiary Insects*.) A glance at these figures will convince the reader that the insects of the Devonian times varied greatly in the structure of their wings. For

[1] *American Naturalist*, vol. xxxii, pp. 233, 234.

these three insects differ as much from each other as do the more generalized members of widely separated orders of living insects. Evidently, comparatively high specializations in widely

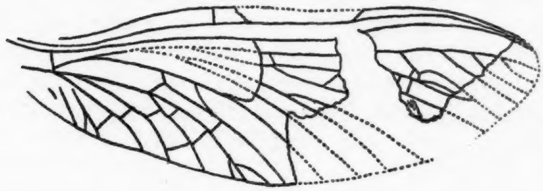

FIG. 71. — *Homothetus fossilis.*

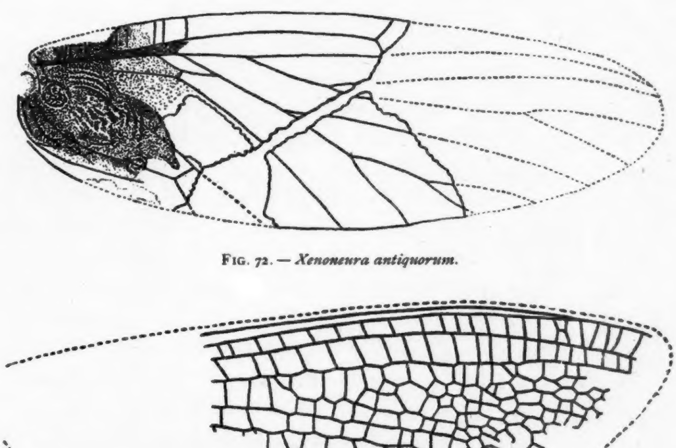

FIG. 72. — *Xenoneura antiquorum.*

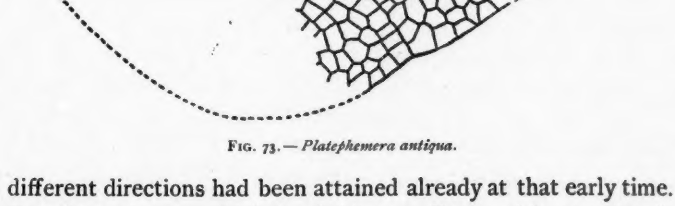

FIG. 73. — *Platephemera antiqua.*

different directions had been attained already at that early time. But the point to which we wish to call especial attention is that, of the three better-preserved Devonian insects, one (*Xenoneura*) had but few wing-veins. And when we consider the

slight amount of data that we have, the numerical preponderance of the many-veined type has no significance.

It is easy to conceive of the development of the wings of all living insects from forms allied to *Xenoneura*, by the different methods of specialization which we have pointed out; for it will be seen that the wing of this insect closely resembles our hypothetical type. And we can say, therefore, that the paleontological evidence does not contradict the conclusions drawn from a study of the ontogeny of living forms.

THE PENEPLAIN — A REVIEW.

R. A. DALY.

ALL workers in Physical Geography are agreed that, given time enough and a constant position of the baselevel, the general processes of denudation on the land will produce, as the result of wearing down any massif, a nearly plane surface approximately coincident with the baselevel of the region. Professor Davis and his followers believe that this ideal condition is represented by actual examples in nature, examples which deviate from the ideal in features that are the expected concomitants of even long-continued denudation. Chief among these is the occurrence of isolated areas of higher ground than the general plain, existent as such because of greater initial elevation of those parts of the massif or because of their being composed of exceptionally hard rocks; they are the "monadnocks" dominating the otherwise nearly featureless plain. That, in such a case, we have to do with an almost-plain, a peneplain, is regarded as none the less certain on account of the presence of these residual hills; on the contrary, they furnish one of the strongest arguments for the fact of denudation. The nearly plane surface of the rest of the massif can be quite independently recognized in the field and on the map. In a recent number of the *American Geologist*[1] Professor Tarr states his opinion that, while conceding the possibility of peneplains on an ideal planet, they do not exist, have not existed, and, if I understand him aright, *can* not exist on this earth of shifting baselevels, moving up and down within time limits represented by one or more geological periods.

Professor Tarr's arguments against the theory of peneplains are both general and special. In the former category are the following: (1) The theory is faulty because it demands too much time. (2) Closely correlated therewith is the objection

[1] June, 1898.

that continental oscillations of level are too pronounced, both from the point of view of amplitude and of frequency, to allow of the amount of beveling requisite to reduce a mountain-built region to the faint relief of a peneplain. (3) There is no known case of a modern extensive peneplain standing at its baselevel. (4) The paper is, however, largely occupied with a concrete argument against the peneplain from an analysis of the facts of land-form in two of the classic regions where the peneplain was first described, namely, in New England and New Jersey.

Before proceeding to a detailed discussion of these various points it is necessary to observe that, in order adequately to demolish the theory, it must be inspected, not only in the writings of the American physiographers, but also in those of European authorship. The peneplain is an American idea, but it has taken firm root in Europe. Men like Penck, Philippson, De Margerie, and De Lapparent are recognizing its truth, and we read of the peneplains of Bohemia, Russia, and the Rhine district; yet we cannot say that these men are specially omnivorous of American geological and geographical conclusions. We may most heartily agree with Professor Tarr in his calling a halt on the wholesale discovery of peneplains on insufficient evidence, but that criticism does not apply to the examples cited.

1. Professor Tarr refers to the difficulty of finding time enough for the process of the peneplanation of a mountainous tract, from the fact that, since the glacial period, there has been sufficient time neither to "strip off the till left by the ice upon the hillsides [of New England], nor to notably modify the very perfect form of drumlins, eskers, and deltas formed when the ice was here." "When we see the slowness of denudation in a hilly country, even a single peneplain seems most difficult to conceive." Is it easier to *conceive* (*i.e.*, actually measure out in the imagination) a stretch of time long enough even to permit of the excavation of the Colorado Canyon or the retreat of the Niagara escarpment fifty miles or more from the former edge of the Niagara limestone, or to produce the bewildering network of valleys in the West Virginia plateau? Whether we

can conceive many geological processes or not does not alter the facts of geology. The velocities of the planets are real, though never adequately conceived by the astronomer. The work has been done — "How?" is the question. To reason truly and effectively about the genesis of a land-form, we must put away from our minds any proneness to measure the scale of the operations by reference to the standards of a human life. It is safe to say that the best work in geology has been done by those observers who have thus put themselves in a sympathetic relationship with the earth and have looked upon her as a great organism whose age is to be evaluated in proportion as her activities become known. The culture of the imagination is an academic bi-product. This argument of Professor Tarr would, then, as seriously militate against *any* important modification of the lands by denudation as it does against that last stage where the forces of erosion have carried it down to a condition approaching a plain. The same criticism can be applied to his illustration of the slowness of geologic changes taken from the behavior of the Penobscot River in time of summer freshet. It is true that there is but little sediment in the running water of the stream, but does Professor Tarr deny that the neighboring mountain, Katahdin, has been worn out of an enormously greater terrane than that represented in the present mountain-root? We may note, in passing, that in any case the actual work of the Penobscot cannot be gauged by mere observations on a summer freshet; it is in the spring, after the frost has loosened débris from the mountain-sides, that most of the transportation is effected for the year.

2. The second objection referred to is more concrete and withal more scientific; yet it may be met in the same way. Each of many of the greater unconformities in the geologic scale means a stand of the land long enough to remove almost completely a mountainous relief of surpassing magnitude by the self-same process of slow denudation as that characteristic of the lands to-day. Witness the pre-Palæozoic land surface of the crystalline shield of Canada, the wonderfully even surface of the Archæan underlying the fossiliferous rocks of European

Russia or of the plateaus of the Colorado Canyon. A striking example of these buried plains of denudation has recently been seen by the writer in that treasury of physiographic illustrations, the Crimea. As the steamer coasts the southwest shore of the peninsula, one sees a dark line running through the cliffs with almost ideal straightness for many miles; this line represents an old land surface on the well-flexed Jurassic, in which the steeply dipping beds are truncated as nicely as if pared down by a huge knife. Upon them lie the light-tinted Eocene sediments, horizontal, and signifying a perfect case of unconformity. Now we believe that this smoothing of the older terrane in each case is capable of explanation, and as yet only two theories have been advanced to serve the purpose. The first, the theory of subaerial denudation, has by far the weight of evidence in its favor; the second, that of marine denudation (sea-benching on a large scale), is in the highest degree improbable, for reasons that need not concern us here. Both theories alike demand a fairly constant relation of land and sea, not absolute quiescence from up and down movement of the land, but comparatively faint oscillations of the land during a long period. If we are going to explain these fossil land surfaces at all, then we are forced to posit a more or less constant baselevel for each. We have, furthermore, in these same regions positive evidence that they have since been capable of a long-continued absence of important movement of the land with respect to their common baselevel, the level of the sea. As far as the wild Archæan tracts of Canada are known, it seems to be the fact that at least one million square miles of that country have remained above the sea during most of Palæozoic time and all of Mesozoic and Tertiary time. Conversely, from the Cambrian to the Triassic, the Russian terrane was sunk below the sea.

3. We must again join issue with Professor Tarr, when he contends that there are no extensive peneplains of modern date existing on the earth. The Russian Plain, from St. Petersburg eastward and northward, is essentially in this condition; while, considering its vast extent, the whole of the European part of the empire has suffered but a minimum amount of warping from

the original position of the same peneplain. But, granted that there be no recent peneplain at or near the present sea level, it would not prove that such plains have not been formed in past geologic ages. Late Tertiary time has been that of exceptional energy in mountain-building on the lands, and very probably in drowning of parts of the continental ridges in the ocean. Both of these correlated causes might well bring about an important lowering of the sea level simultaneously about all the continents. The result of such movements would be to raise some of these plains above their ideal position near the sea, to tilt others either toward or from the sea, and, possibly, to leave one in about its original attitude with respect to the ocean level.

4. Professor Tarr goes farther and denies that there has yet been adduced evidence that there exist peneplains of ancient date, *i.e.*, those which have been uplifted and are being dissected in a new cycle of geographic development, or those that have been buried in sediments after depression; the peneplain theory is useless because there is no peneplain to need explanation. To establish this doctrine he gives us the results of his study of the uplands of New Jersey and New England.

He finds five hundred feet of difference in the tops of the New Jersey hills, and about as much in "the very even-topped Kittatinny Mountains." "There is a very distinct lack of uniformity in the elevation of the upland crests"; it is truly a distinct lack, but is it, even on the showing of Professor Tarr's figures, sufficient to remove the topographic facet represented in the average level of these higher points from the category of an almost-plain? He tabulates the ranges of elevation on the survey sheets of Connecticut as follows:

Cornwall	1787–1215	feet,	a range	of	572	feet.
Winsted	1600–1160	"	"	"	440	"
Granby	1240–720	"	"	"	520	"
Hartford	lowland.					
Tolland	985–660	"	"	"	325	"
Woodstock	761–540	"	"	"	221	"

He concludes that it is more important to emphasize the total range of elevation of the crests in this 91 miles than to

emphasize the fact that in any given sheet the total range is relatively very small and decreases with the decreasing absolute heights of the hills in going from west to east. Is this striking accord of elevations to be ignored in an explanation of Connecticut scenery? Professor Davis has regarded it as indicative of a peneplain of the last geographic cycle, now tilted towards the east and somewhat dissected, especially on the softer rocks, in the present immature stage of a new cycle. It is a theory at once clear, definite, and involving no processes other than those actually illustrated on the earth at the present time. It explains the existing uplands and valleys of the regions under discussion, and, above all, the presence of this otherwise inexplicable topographic facet which is once more established by the foregoing table of Professor Tarr.[1]

A second point that he makes against the New England peneplain shows a disregard of patent facts which it is most difficult to understand in the most superficial survey of New England geography. He writes: "In Maine, New Hampshire, Vermont, western Massachusetts, and the Adirondack region, with similar structure to that of the region above mentioned, and so near them that they must have been subjected to the same general degradation, the lack of uniformity of upland crests is very much more marked." The elevation of the latter "is 2000 to 4000 feet above the sea." Now, so far as the present writer is aware, neither Professor Davis nor any one else has contended that these summits represent any part of the peneplain; they are, on the contrary, stated to be local and regional monadnocks interrupting the general surface of the peneplain for very good reasons. Among the latter is one which has met with contradiction in an expression of the passage just quoted, namely, that there is similarity of "structure"

[1] I should not hesitate to speak of an extensive mountain range as a "peneplain" if it no longer showed a range of elevations greater than 572 feet in a stretch of 91 miles. It is, however, true that the ranges of elevation cited by Professor Tarr are determined from the position of streams which have strongly incised their beds because of the Tertiary uplift. On the other hand, some of the higher points may represent extremely low monadnocks overlooking the peneplain of Professor Davis's definition (range of elevations through 200 or 300 feet) from altitudes of 100 or 200 feet.

between these terranes and that underlying the peneplain. Whether "structure" means "position" alone or "position" plus "texture" (as it manifestly ought not to do), we cannot agree with the statement. The granites, granitites, syenites, and quartz porphyries of Osceola, Tripyramid, the eastern Kearsarge, Red Hill, and the New Hampshire Whiteface, are entirely different in structure and mineralogical characters from the foliated rocks of Massachusetts or even the batholites of coarse porphyritic granite of New Hampshire and Massachusetts, and we believe that no practical worker in the field relations of these areas finds difficulty in putting in similar contrast the Massachusetts foliated rocks and the heavier, more massive gneisses and schists of the White Mountains proper. Is the structure of the Adirondack granite massif "similar" to that of the Berkshire plateau, the structure of Mount Ascutney, Vermont, with its three great stocks of deep-seated intrusives, "similar" to the structure of the surrounding phyllites and gneisses, or that of the granitic Katahdin "similar" to the Calciferous slates through which the granites came?

Professor Tarr goes on to say that he has stood on the "higher" peaks of Maine and looked in vain for any series of peaks that even to the eye appeared uniform in level. For any one to advance this observation as an argument against the New England peneplain seems to us quite incomprehensible. To look for a fairly chosen peneplain sky-line a thousand feet or two thousand feet above the level at which the advocates of the peneplain ask us to find it, and to look for that sky-line in a nest of granitic monadnocks, is hardly likely to be the mode of procedure by which to successfully attack the theory of the peneplain.

He questions any serious lack of "sympathy between the level-topped hills and the rock texture and position." He notes the fact that the soft gneisses and the limestones of New Jersey lie in regions of average lowland, and the hard gneisses underlie areas of greater elevation. This is the sole argument he makes to support this lack of sympathy. He then states, without the shadow of proof, that, "although the rocks are complex in kind and position, they now stand in very general

harmony with topography." In the next paragraph he admits that "the region is a lowered mountain mass, evidently once of a very rugged topography, but now much reduced and traversed by drainage lines of a somewhat mature form, the result of elevation." But that there be or not a lack of sympathy between structure and land-form will evidently depend, among other conditions, upon the degree of the lowering. The make-up of the New England upland certainly indicates the former existence in the district of massifs of flexed rocks at least as imposing from their altitude as the modern Alps (one resulting conclusion from the detailed work of Emerson in western Massachusetts, of Shaler and Woodworth in the faulted and folded region of Narragansett Bay, of Pumpelly, Wolff, and Dale in the Green Mountain axis, to say nothing of the older work of the Hitchcocks and others). Where are the ridges corresponding to the folds of the Boston Basin, or the fault-scarps of Rhode Island or of the Connecticut Valley? It is, in other words, impossible to speak of sympathy between the topography and the structures of New England, when the latter demand displacements of thousands of feet, especially in view of the fact that the valleys and hills of the upland run at all angles to the axes of folds or the lines of fault. There *is* some sympathy between topography and the hardness of the rock-members, but it is only with the extremest rarity that there can be traced any correspondence of structure (position) and the land-form in New England outside of the Narragansett Basin or the Triassic lowland.

The evident adaptation of many of the valleys and broader lowlands of New England to the superior softness of the rocks they cover is just what we should expect to find on any theory of New England topography that recognizes a period of uplift, recent, but long enough ago to allow of the excavation of the valleys to their present levels. This uplift Professor Tarr acknowledges, but claims that "there is no evidence to prove" that this uplift has been differential, that is, that there has been a tilting toward the southeast. If there has been no such tilting, it is necessary, following Professor Tarr, to consider that while the Deerfield has cut its canyon-like valley, and is still

cutting downward with energy, the eastern part of Massachusetts has been lowered to a degree where, in Professor Tarr's words, "there may well have been an approach toward the condition of a local peneplain." That is, the relatively insignificant amount of work necessary to hew out the Deerfield valley must have taken as much time as the wasting to an almost-plain of a coastal belt from twenty to thirty miles broad at least. From his own point of view, Professor Tarr would explain this strong contrast in the rate of cutting by the fact that the coastal region is the locus of river systems with short courses to the sea, and, consequently, great head and erosive power. But how is it that the plateau of western Massachusetts is not similarly worn down by the rivers which have a quick course to the sea *via* the long-opened-up Triassic lowland on the west? (This implication, in the last quotation, that subaerial erosion may produce a peneplain near the coast on the scale permitted by the range of elevations in Massachusetts, seems to be a virtual abandonment of his whole position on the part of Professor Tarr.)

The theory of differential uplift is antecedently probable, and the facts of present geographic form are exactly those that would be expected to result from the elevation of New England at the close of a former complete cycle of denudation. A tilting toward the east and south would give the revived rivers of the northern and western parts of the peneplained region power to etch out of the terrane narrow valleys on the hard rocks and broader valleys on the less resistant ones. The broadening of the valleys would progress as the time allowed, as the area of soft rocks permitted, and as the river was strong. Where the second and third conditions are satisfied, as in the case of the Connecticut, we have a broad open (Triassic) lowland of the new cycle; when the second is not so well fulfilled, the same river gives us a much narrower lowland, that on the "Calciferous mica-schists" of southern and central Vermont.

One of the most strongly emphasized objections of Professor Tarr to the theory of peneplanation is based on the occurrence of monadnocks, or residual hills, on the New England upland. He asks: "Are the monadnock rocks essentially harder than the other hilltops of the neighborhood?" and answers the ques-

tion in the negative on page 363 of the article under review, and in the affirmative on page 369.[1] If an affirmative answer be not possible, there is no possible explanation of these higher crests in the light of our present geological knowledge. Monadnocks like the Blue Hills near Boston, Ascutney Mountain, Chocorua, Katahdin, are made of plutonic rocks, necessarily crystallizing under the pressure of overlying masses of the same lithological character as the rocks now seen in the immediate vicinity of these eruptive centers. The overlying rocks are gone, the surrounding rocks are well beveled down; the stock-rocks stand up as knobs — Why? — because they are harder. If there can be suggested a simpler, more probable explanation, it is high time that geologists should find it out.

It has been my fortune to map geologically one of these typical monadnocks, Mount Ascutney, and, on all sides of it, to meet with striking illustrations of the fact that it stands above the general level solely because it is more resistant to destructive agents. The streams running across the syenites and granitite far up the mountain are slowly cutting their gorges, continued in radial arrangement, across the contacts on all sides. At the contacts there is almost universally an abrupt fall where the streams escape on to the thinly foliated schists or basic intrusives that encircle the main mountain. This exhibition of differential hardness is correlated with an only less important steepening of slope where not the perennial streams, but the general process of wasting and wash, "creep," has developed a sudden fall-off just at the zone of contact of intrusive and country-rock. In the reconnaissance of the mountain this latter slope was used as a rapid means of locating the contact.

Now *these* are the test cases. It is, indeed, difficult to say why the gneissoid and schistose rocks of Mount Monadnock have refused to weather down as rapidly as the similar schists round

[1] On page 363 we read: "I believe that I am correct in saying that there are no very distinct differences between the rocks of the monadnocks and the lower hills, in point of durability." On pages 368 and 369 we have: "There will be a beveling of hilltops where the harder gneissic and granitic rock exists, the stream valleys standing near the base level, and hills of softer strata standing at levels still lower, in which the rock is harder. . . . Can any evidence be adduced to show that New England has ever advanced further in development than this stage?"

about; but, by all the laws of analogy, it is simplest to believe that the same explanation of differential hardness applies here. (I would go so far as to say that, on the basis of direct observation in the field, it is not differential *elevation* that explains the existence of Mount Monadnock.)

Following his criticism of the peneplain theory, Professor Tarr attempts some constructive theorizing on the present relief of such regions as New England and New Jersey. He regards it in each case as a mountainous area in the condition of "full maturity of topography," "reduced mountains, lowered to the stage of full maturity." "By this explanation it is held that the region was never reduced to the peneplain stage, but has always been, as it still is, a mountainous section, though once less mountainous than now, because of the recent uplift." But, disregarding the uplift, is it correct to speak of such relief as that of Massachusetts as characterized by a stage of "mature" dissection? Will the average inhabitant of the upland be flattered in learning that the tract in which he lives ought, by the laws of distribution, to be occupied by a few miserable, impoverished individuals, isolated by a labyrinth of valleys and hills from their fellows? For such is the condition of most of the maturely dissected areas of the earth's surface; the moonshiners of West Virginia eloquently represent one result of mature dissection. Fortunately for the needs of man, a maturely dissected region is almost as rare as one that is characterized by chronic earthquakes of destructive violence. But when, in addition, Professor Tarr states that, by "mature" topography he did not mean even the present relief, but a topography made "more rugged" by the recent uplift, it is all the more difficult to subscribe to his nomenclature of that earlier surface. So far is the Berkshire plateau, for example, from being of "mature" form, made more rugged by uplift, that an unprofessional observer, returning from a walking tour across the plateau, has remarked to me in very definite language that the plateau is "flat," so strong was the impression upon him of the even-topped character of the whole. Yet how likely is the untrained student of land-forms to overlook the existence of a topographic facet, however faintly it may be dissected!

The most valuable part of the paper is that in which Professor Tarr emphasizes the effect of the tree-zone in retarding the progress of denudation and the consequent tendency of the higher summits of a mountain range. Pike's Peak is a good example in point, the rounded slopes below that zone contrasting finely with the serrate spurs of rapidly degrading naked rock far up. Professor Tarr would find in the halting of one peak at the snow-line until overtaken in the downward journey by its neighbors an explanation of the even tops of "mature" mountains, in particular those of New England. But the very rough accordance of levels expected in this ideal scheme, and represented in parts of the Alps and Carpathians, is not in the slightest degree comparable with the New England conditions, nor with the "very even-topped Kittatinny Mountains," referred to in the article. Thus it is impossible to conceive that the New England facet can be accounted for as formed at a vanished tree-zone. Having reached the tree-zone, the forces of denudation then tend to reduce the mountains still further, but now according to the law of soft and hard. Now, since the effective tree-zone may be roughly put at an elevation of between 4000 and 5000 feet in New England, we are as badly off as ever in arriving at a conception of how a nearly plane surface can be produced on the rocks of differing hardness composing the mountains, unless that surface is the result of the beveling down of all the terranes nearly to baselevel. Finally, there are well-established peneplains, such as that on the old rocks of Missouri, where the tree-zone theory cannot avail, because the district was undoubtedly never raised, at any time during the geographical cycle represented, to a height that would bring about the differentiation of a tree-zone. It is an old plateau of horizontal or but gently folded rocks.

HARVARD UNIVERSITY, December 10, 1898.

A PECULIAR TOAD.

F. L. WASHBURN, A.M.

In 1896 Mr. J. R. Wetherbee, a student in our biological laboratory, was the recipient of a curious specimen, a toad (*Bufo Columbiensis* Baird and Girard) having an extra arm projecting at an angle from the left side just in front of the normal left arm. The species is not uncommon in parts of

Oregon, but the finding of one with a fifth limb is of rare occurrence, possibly unknown hitherto. The abnormal arm was 3 centimeters in length. Apart from this peculiarity the specimen appeared and acted like any other toad, apparently in no way inconvenienced by this extraordinary lavishness of nature. The extra arm was supplied with 7 digits, and, though not provided with an elbow joint, it could be moved and was moved to a slight extent at the proximal joint next the body.

A photograph of this curious animal was taken by the author and is reproduced on the preceding page.

A drawing of the pectoral girdle and fore limbs is reproduced below (Fig. 1). The ulna and radius of the abnormal arm are separate bones, not fused as they are normally; the proximal

FIG. 1.

end of each is attached to the humerus by intervening cartilage; there are no distinct carpalia, but the metacarpals are joined by cartilage directly to the two long bones, and are grotesquely noduled at the proximal end: 1, sternum; 2, clavicle of right side; 3, coracoid of right side; 4, normal coracoid of left side; 5, normal clavicle of left side; 6 is opposite abnormal coracoid; 7, over abnormal clávicle; 8, humerus of abnormal arm; 9, 10, abnormal radius and ulna; 11 is over scapula and supra scapula of right side; 12 is the left scapula.

The following sketch (Fig. 2) by Mr. Wetherbee, and kindly

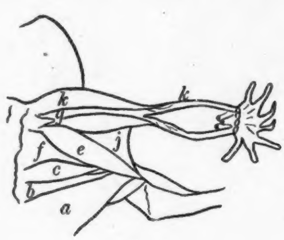

FIG. 2.

loaned for this article, was made from his dissection of the superficial muscles of the parts in question. Both Figs. 1 and 2 disclose peculiarities of internal structure which one would expect to find from a consideration of the exterior. There is,

apparently, in connection with the abnormal arm a duplication of some of the muscles of the chest and normal arm.

a = abdominal portion of pectoral muscle.

b = posterior sternal portion of pectoral muscle.

c = anterior sternal portion of pectoral muscle.

f is evidently intended by Mr. Wetherbee to represent the normal sterno-radialis, and *g* is probably the duplicate of *f* for the abnormal arm. The letter *g* is not distinct in the cut; it lies just below *k*.

k, *e*, and *j* I am in some doubt about from Mr. Wetherbee's description, and hesitate to name them. Quoting from his notes: "*k*, origin anterior edge of abnormal arm and a few fibres on sternum, insertion *k'*; *e*, superficial, origin precoracoid, insertion humerus; *j*, attached to dorsal surface of abnormal arm and to under surface of normal arm."

He also says in his notes: "The infra spinalis muscle was nearly twice normal size, and was inserted by two tendons to the fifth arm and [by] one to the normal arm."

The blood system did not offer enough peculiarities to warrant a reproduction of the drawing; the in-going and out-going blood of the abnormal arm passed through extra branches of the subclavian artery and subclavian vein, respectively.

Biological Laboratory,
University of Oregon, Sept. 20, 1898.

HUMAN REMAINS FROM THE TRENTON GRAVELS.

DR. FRANK RUSSELL.

IN the long controversy regarding the age of the implements or "paleoliths" from the Trenton gravels little attention has been given to the human remains from the same beds. Believing that some account of them would prove to be of general interest, I have undertaken[1] their study in the attempt to determine whether or not they resemble the remains of recent Indians of that region. As the Delaware Valley was occupied by the Lenni Lenapé until 1737, the crania found near the surface, at least about Trenton, are, presumably, of members of that tribe.

Material. — The most interesting of these relics is an imperfect calvarium (Fig. 1) found at a depth of twelve feet from the surface in the stratified gravels. It was discovered by a laborer who was leveling the bottom of the pit which had been dug for the gasometer of the Trenton Gas Company. The workman's spade cut away a large portion of the left parietal, which was not recovered. The skull was taken by the foreman in charge of the excavation to a druggist, who displayed it in the window of his store, where it was seen by Dr. Abbott and obtained by him for the Peabody Museum at Cambridge. Both the foreman and the druggist are now dead, so that no statement can be obtained of the circumstances, though diligent inquiries were made at the time by Dr. Abbott. As no one received any compensation for the skull, there was no evident motive for deception. This skull, together with all the specimens described in this paper, is now in the possession of the Peabody Museum of Harvard University. In the report of the curator of the museum, F. W. Putnam, for 1879, we find this statement: "The

[1] With the kind permission of Prof. F. W. Putnam. I have also to thank Dr. C. C. Abbott and Mr. Ernest Volk for information about the position of the specimens.

two human crania received from Dr. Abbott are of particular interest; one probably being the skull of a Shawnee Indian, while the other, which is of entirely different shape, small, long, and very thick, was found in the gravel under such circumstances as to lead to a belief in its very great antiquity."

The calvarium (Fig. 2) from Burlington County, New Jersey, was found by Michael Newbold in 1879, while plowing a field

FIG. 1.

where the "gravel came to the surface." It would have been classed as that of a recent Indian had it not been found in the gravel, not accompanied by the remainder of the skeleton, and had it not resembled the Trenton skull in being low vaulted.

Another calvarium (Fig. 3), exhibited with the two preceding and usually regarded at the Peabody Museum as of the same type, was found in Riverview Cemetery at Trenton in 1887. The workman who found it states that the skull was lying two and one-half to three feet from the surface in clear greenish sand. "The place where it was found is on a knoll, one of the highest

points in the cemetery. No other bones were found with it. There were a few black lines near the skull; they may have been caused by the decayed roots of trees, or bones. No trace of black soil was noticed with the sand. Numerous Indian relics were noticed in the top soil." [1]

Three fragments have been found by Dr. Abbott himself, in the cuts made by the railway in the stratified gravels at Trenton. One of these is a left temporal bone (Fig. 4), the petrous portion of which is broken; and its whole appearance

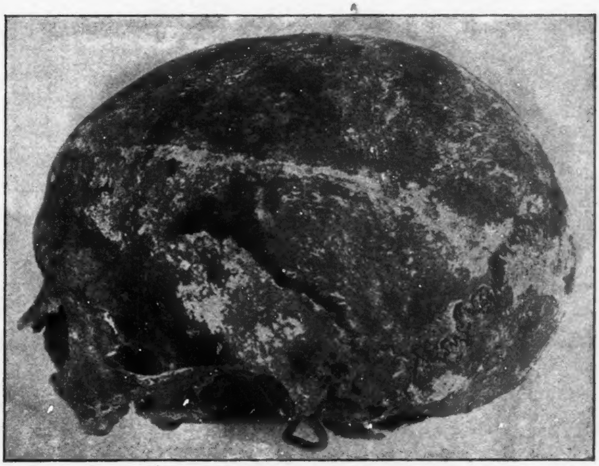

Fig. 2.

strongly corroborates the statement of the finder, that it was taken from the undisturbed glacial gravels at a depth of thirteen feet.

A portion of the left ramus of a human jaw (Fig. 5) was found by Dr. Abbott at the same locality in 1884. It was lying at a depth of sixteen feet from the surface and appears to have been subjected to rough usage by the gravels. The jaw is that of an individual, having a prominent chin, and exhibits neither primitive nor simian characters.

[1] Statement made recently to Mr. Volk and communicated in a letter dated Dec. 7, 1898.

Still another specimen found by Dr. Abbott at this place is a human tooth, a third molar, an account of which was given in a paper read by him before the Boston Society of Natural History in October, 1882.[1]

In the attempt to identify the single skull, or, assuming that the Burlington and Riverview specimens are also ancient, the three skulls from Trenton, we must necessarily proceed by the

FIG. 3.

comparative method. A number of Indian skeletons have been discovered by Mr. Volk during his excavations upon the Lalor Farm at Trenton, but these specimens are "in boxes and not accessible." The only crania from that region available for comparison with the Trenton skulls are one from West Chester, Penn., in the Delaware Valley, and one from a Delaware peat bog.

I have also included in the table of measurements the averages of a series of five from the stone graves of Tennessee, selected from a large number as being nearly if not quite free

[1] *Proceedings Boston Society of Natural History*, vol. xxii, p. 96.

from artificial deformation. To the northward of the region in question but two crania are available, and these from Central New York, probably those of Indians belonging to the Iro-

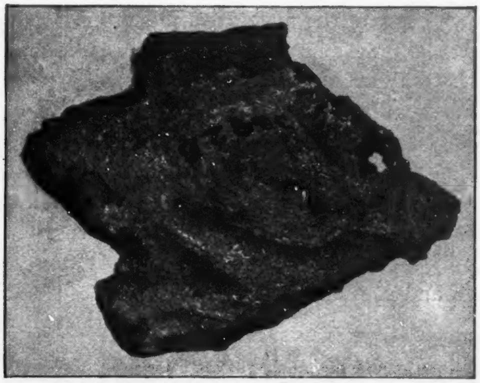

Fig. 4.

quoian stock. From the east a series of Massachusetts crania is taken. For additional comparison a series of skulls from Santa Cruz Island, off the coast of California, is introduced; these have the same cranial index as the average of the three

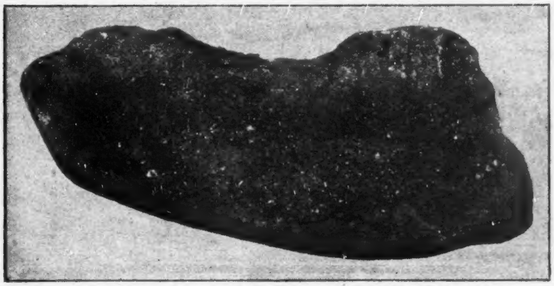

Fig. 5.

from New Jersey. Accounts of most of these have been published by Mr. Lucien Carr, the accuracy of whose measurements is unquestioned; yet I have re-measured all because of some differences in methods of procedure, and in order to eliminate,

as far as possible, the probability of error arising from the personal equation in general.

Discussion of the Measurements. — A general similarity will be noted in the absolute dimensions of the individual crania, except in the case of the male calvarium from Delaware, which is somewhat larger than the others. The condition of the three New Jersey specimens is such that the capacity cannot be gauged by the usual methods. However, the Trenton skull, the smallest, is neither so short, so narrow, nor so low as two adult crania in the Massachusetts series. In all transverse diameters the Trenton skull closely resembles the modern skull from West Chester (Fig. 6), while in the sagittal diameters and the projections from the auricular axis it stands nearer the Burlington and Riverview skulls. The marked brachycephaly

FIG. 6.

of the Burlington skull (Fig. 7) is rare among Algonquian crania, and upon the evidence of this character alone the investigator is inclined to exclude it from further comparison with the Trenton

skull. The Riverview skull (Fig. 8) has an open metopic suture and is both lower and broader than the Trenton specimen.[1]

An exhaustive comparison of the absolute measurements would lead to nothing; the indices are more suggestive. The

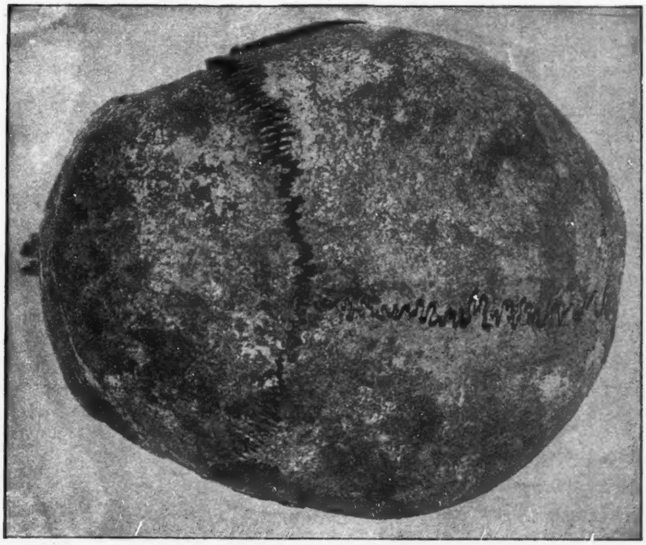

Fig. 7.

cranial index is, of course, the most important of these. In this respect the Trenton skull (Fig. 9) stands near the mean of the whole numbers represented in the table of measurements. The average cranial index of four female crania from Maryland,[2] described by Cope, is 75.1. These crania "are inferably those of Nanticokes,"[3] closely related, of course, to the Lenni Lenapé of the Delaware Valley. The index of the Trenton skull, 77.8, is well within the limits of mesaticephaly and is about the aver-

[1] Harrison Allen has described a Lenni Lenapé metopic cranium having a cranial index of 75, vertical 76, orbital 92, and nasal 53, *Crania from the Mounds of Florida* (p. 408).

[2] Cope, E. D. *Physical Characters of the Skeletons found in the Indian Ossuary on the Choptank Estuary, Maryland* (p. 99).

[3] Mercer, H. C. *Exploration of an Indian Ossuary on the Choptank River, Dorchester County, Maryland* (p. 98).

age of Algonquian female crania; the broken parietal gives it a deceptive appearance of narrowness. Its vertical index (72.5) resembles that of the West Chester skull, and is widely removed from those of the Burlington and Riverview specimens. The relative height is not notably less than that of the Massachusetts series, in which the range of this index is from 67.8 to 8.5. The superior facial index could not be calculated in many cases because of the broken condition of the crania. The naso-malar index, which expresses the degree of projection of the interorbital region, is quite uniform and within the limits of individual variation for the whole series. The racial averages given by Thomas[1] are:

9 Mongols	105.9,	range	105.1	to	106.9
5 Andannanese	107.5	"	105.5	"	108.6
25 West African Negroes	108.5	"	106.1	"	113.3
16 Caucasians	111.1	"	109.1	"	114.2
Sex not stated.					

ADDITIONAL EXAMPLES OF AMERICAN CRANIA.

10 ♀ Massachusetts Indians	108.6,	range	105.6	to	111.2
16 ♀ Santa Cruz Island Indians	107.4	"	101.1	"	111.4
5 ♀ Tennessee Indians	108.4	"	106.3	"	110.2
15 ♀ Labrador Eskimos	105.8	"	104.1	"	108.2

The orbital index is higher in the Trenton skull than in the Eastern Algonquins, in only one of which is the index above 90. In the shape of the orbit the New Jersey and West Chester skulls stand apart from the others and fall within the megaseme group. Unfortunately, the nasal index cannot be calculated for the Trenton skull; the West Chester and Riverview crania both have lower nasal indices than the skulls from the region north and west of them. The evidence of the remaining indices is inconclusive and comment is unnecessary.

Condition of the New Jersey Crania. — The sutures of the Trenton skull are obliterated, and its thickness is sufficient to insure its preservation where an ordinary skull would be crushed to fragments. However, it does not present the appearance of having been rolled about for any length of time in the gravels; the left styloid process projects for a distance

[1] Oldfield, Thomas. *Journ. Anth. Inst.*, vol. xiv, p. 333.

of 10 millimeters (Fig. 1), and the surface of the brain-case is almost without a scratch. The lower portion of the face has been broken away in a manner similar to that seen in many skulls from recent graves, and not directly through the stronger parts of the bone, as in the case of the Calaveras skull. The worn appearance of the margins of the orbits and the portions that remain of the zygomatic arches may be ascribed to the

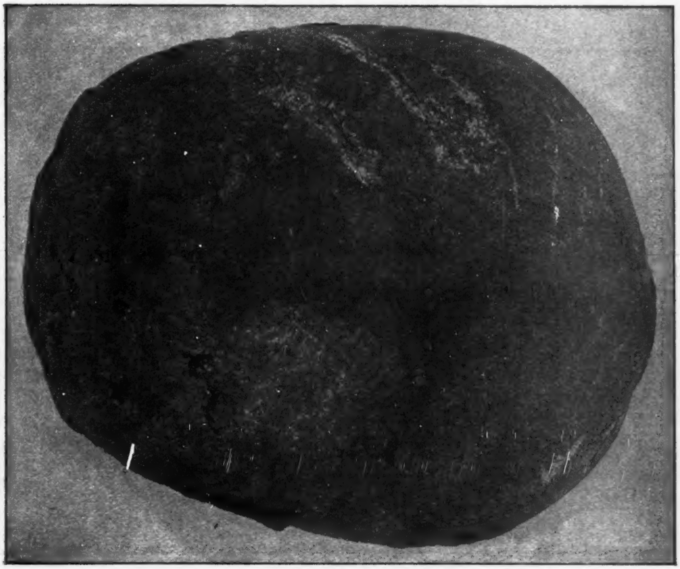

FIG. 8.

vicissitudes of a brief journey in the waters of the Delaware River or Assunpink Creek. Though the surface of the ground where the skull was found is twenty feet above the ordinary level of the Delaware, the locality has been overflowed in recent years, so that existing agencies could have swept skull and gravels into place and buried them beneath succeeding strata of sand and gravels and huge ice-rafted boulders. The length of time that has elapsed since the skull was deposited in the gravels is probably very great, though of course it is not geologically ancient. The presence of the fragments of crania

in the gravel pits along the railway may be accounted for by the same hypothesis advanced by Dr. Abbott to explain the position of the rude implements in that locality. The railroad cutting passes through an old channel of Assunpink Creek, and Dr. Abbott states that "it was *always* along the old creek bed that the chipped argillites were found, along the railroad excavations east of the site of the gas works where the skull was found; and when the excavations continued *beyond the*

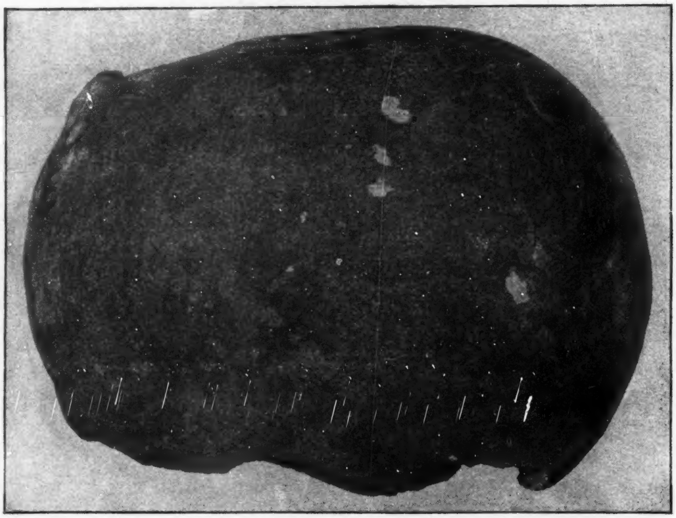

FIG. 9.

area of the immediate creek valley they disappeared."[1] The thickness of the Trenton skull is not rare in American crania. The Delaware skull, No. 48,974, furnishes an example close at hand of an even greater thickness.

The Burlington and Riverview skulls are thin and fragile, and their sutures are open (Figs. 7, 8). They are unworn and present no evidence of ever having been moved by flowing water, torrential or otherwise. They do not closely resemble the Trenton skull, though the morphological differences are

[1] In a letter to the writer, December, 1898.

well within the limits of variation of a tribal group. The three skulls are distinguished from those from the regions to the north, east, and west, and are related to the skull from West Chester by their orbital and nasal indices and minor characters.

Conclusion. — From the evidence supplied by the Trenton skulls themselves the conclusion is inevitable that they are of modern Indians, probably of the Lenni Lenapé.

TABLE OF MEASUREMENTS IN MILLIMETERS.

LOCALITY	Briar Hill, St. Lawrence Co., N. Y.	West Chester, Penn.	Cayuga Co., N. Y.	Peat Bog, Del.	Trenton, N. J.	Burlington Co., N. J.	Riverview Cemetery, Trenton, N. J.	Averages. Eleven from Santa Cruz Island, Cal.	Averages. Sixteen from Massachusetts.	Averages. Five from Stone Graves, Tenn.
Catalogue No.	14114	19512	38964	48974	14635	19513	44280	—	—	—
Sex	♀	♀	♀	♂	♀	♀	♀	♀	♀	♀
1. Length, glabello-occipital	180	180	174	193	171	176	185	174.1	178.1	169
2. Breadth	134	130	135	142	133	147	146	136.9	134.5	130.8
3. Minimum frontal breadth	95	93	87	98	94	94	96	89.7	90.9	90
4. Bi-asterial breadth	105	105?	91	119	—	—	—	107.7	106.8	99.5
5. Bi-auricular breadth	118	113	111	123	115	112	118	—	—	—
6. Bi-zygomatic breadth	—	—	124	140	—	123	—	128	127.4	—
7. Naso-malar breadth	107	108	101	115	104	103	105	104.5	102.6	105
8. Bi-malar breadth	99	98	92	108	96?	93	98	96.2	95.5	96.8
9. Bi-maxillary breadth	97	97	91	103	—	92?	93?	94	92.8	93.3
10. Bi-alveolar breadth	61	67	—	70	—	—	57?	63	63.4	65
11. Maxillary length	55	55	57	55	—	—	51	53.8	53.3	54
12. Basi-alveolar length	96	105	103	105	—	—	80	100.8	95.3	97.6
13. Basi-nasal length	93	101	104	105	93	95	84	101.6	92.6	99
14. Basi-bregmatic length	125	132	134	137	124	119	118	123.8	134.5	134.4
15. Basion-obelion	122	130	125	138	124	118	122	121.2	129	126.2
16. Basion-lambda	116	121	115	125	111	111	120	112.4	118.3	112.6
17. Basion-opisthion	—	—	35	42	33	37	37	34.2	34.9	32.3
18. Breadth of foramen magnum	—	—	30	32	26	28?	32	29.3	31.3	28
19. Orbital breadth	41	40 L	37	42	37	37	36	38.7	39.2	39.2

20. Orbital height	33	37	32	35	35?	36	33	33.8	36.8	34.4
21. Orbital depth	51	51	51	52	50?	48?	50?	48.1	49.7	49
22. Inter-orbital breadth, bi-dacryic	20	23	20	24	24	20	25	20.2	19.3	21.5
23. Nasal height	51	52	50	44	—	—	51	46.4	51	49.4
24. Nasal breadth	26	24	27	30	—	—	25	22.4	25.8	26
25. Palatine length	51	54	54	55	—	—	47	45.9	47.1	48.5
26. Palatine breadth, anterior	22	24	—	27	—	—	23	24.8	25.8	26.8
27. " " posterior	38	43	—	46	—	—	—	40.4	41.6	42.2
28. Naso-alveolar length . .	73	72	72	63	—	—	69	63.8	68.3	67.8
29. Maximum circumference	506	498	499	550	504?	514	535	495.2	498.4	481.6
30. Supra-occipital arc . .	400	392?	384	432	353	386	400	388.7	398	380
31. Supra-auricular arc .	293?	298	299	312	284?	300	298	282.4	288	292.2
32. Ear to inion	78	—	75	88	83	73	82	—	—	—
33. Ear to vertex	108	117	114	122	107	105	105	—	—	—
34. Ear to glabella	93	94	97	99	85	92	87	—	—	—
35. Ear to orbit	65	67	69	70	60	62	58	—	—	—
36. Facial angle	80°	80°	83°	84°	—	—	84°	78.5	81.9	82°
37. Thickness	—	—	—	8–11	8–10	—	—	—	—	—
Indices.										
38. Cranial	74.4	72.2	77.6	73.6	77.8	83.5	78.9	78.7	74.8	77.4
39. Vertical	69.4	73.3	77	71	72.5	67.6	63.8	71.1	75.9	79.5
40. Superior facial, Broca .	—	—	58	45	—	—	—	50.3	—	—
41. Naso-malar	108.1	110.2	109.8	106.5	108.3	110.8	107.1	107.4	108.6	108.4
42. Orbital	80.5	92.5	86.5	83.3	94.6	97.3	91.7	87.7	86.3	87.7
43. Nasal	51	46	54	68.2	—	—	49	48.3	50.6	52.7
44. Maxillary	110.9	121.8	—	127.2	—	—	111.7	119.4	118.6	120.5
45. Palatine	74.5	796	—	83.6	—	—	—	88	88.1	87.5
46. Foramen magnum . . .	—	—	85.7	76.2	78.8	75.7	86.5	85.7	86	85.1

EDITORIAL.

THE department of scientific investigation of the United States Fish Commission has awakened to new life under the able supervision of Professor Bumpus, and it aims to aid science in every way not incompatible with the fisheries industry. The laboratory at Woods Holl is to be kept open throughout the year, and students are welcomed there at any time. The facilities of the various stations are placed at the command of those who wish embryological or other material. In the line of research we learn that the department has arrived at the conclusion that the late increase in the number of starfish in the oyster beds of southern New England, and especially in Narragansett Bay, is directly related to the seining of the menhaden and other fishes for the oil and fertilizer factories. These surface feeding fishes formerly destroyed large numbers of starfish eggs and larvæ, but since they have been fished so persistently, the starfish have got the upper hand. It is incumbent upon Rhode Island, if it wishes the best for its oyster industry, to place some restrictions upon the foreign corporation at Tiverton.

REVIEWS OF RECENT LITERATURE.

ANTHROPOLOGY.

Report of the Smithsonian Institution.[1]—The report for 1896 contains an account of the "Pueblo Ruins" near Winslow, Arizona, by Dr. J. Walter Fewkes. The exploration of these ruins brought to light a large number of specimens of great interest and value, several of which are depicted in the accompanying illustrations. The character of these plates is noteworthy; they represent the art motives and the symbolism of the ancient Hopi rather than the colors and general appearance of the objects. The small but well-made specimens of turquoise mosaic figured in this paper suggest the more elaborate turquoise-covered objects from ancient Mexico. The chief interest in these investigations arises from their bearing upon the migration legends of the Hopi; we cannot but believe that the examination of the numerous skeletons collected would supplement and enhance the value of Dr. Fewkes's conclusions.

A second paper of anthropological interest is by Professor Hermann Meyer upon the "Bows and Arrows of Central Brazil." This is announced to be an introduction to a larger treatise in preparation. The author based his work upon the ethnographic material contained in the museums of Northern Europe, and deplores the well-known difficulties in the way of ascertaining the exact place of origin of such specimens. A number of illustrations and a map of the central portion of the continent show the distribution of the several types of bows and arrows found in that region. As a preliminary paper this is suggestive and helpful, but with such scanty data the author's deductions regarding the ethnographic character of the Mato Grasso peoples would seem to be liable to considerable modification as the investigation proceeds.

Report of the Anthropological Work in the State Institution for Feeble-Minded Children.[2]—This paper of nearly a hundred

[1] *Annual Report of the Board of Regents of the Smithsonian Institution, 1896.* Washington, Government Printing Office, 1898.

[2] *Forty-Eighth Annual Report of the Managers of the Syracuse State Institution for Feeble-Minded Children, for the year 1898.* Supplement by Dr. Ales Hrdlička. Syracuse, N. Y., State Printers.

pages embodies the results of an examination of the case-book records of the Syracuse Institution and of the Newark Asylum for feeble-minded women, together with the physical examination of patients with reference to reflexes and structures in the mouth. A comparison is made between the normal birth rate and that in the feeble-minded for the several months of the year. Data are wanting for the state as a whole, but the records of New York City show a decided maximum of births in August, while among the feeble-minded the maxima are in May and December. The proportion of feeble-minded is not materially affected by immigration. The numerical position of the patient in the family is of interest as the proportion of first or only children is above the average. The etiological factors receive careful consideration, and in his concluding remarks the author advocates the employment of men for the purpose of gathering information regarding the history of each individual admitted to these institutions. There can be no doubt but that the accumulation of such statistics would contribute toward the amelioration of the condition of these unfortunates, and possibly to legislation that would remove in some degree the "varied and numerous" causes of the mentally defective. A careful study of this class will also show its intimate relation to the criminal and other abnormal classes; "it will demonstrate the fact that the feeble-minded are simply a link in the great chain of the degenerate class and not an isolated class by itself."

Anthropological Notes. — In the November number of the *American Anthropologist* is published an article entitled "Study of the Normal Tibia," by Dr. Ales Hrdlička, which gives the results of his examination of over 2000 normal adult bones. In addition to the study of the degree of variation in the individual, between the sexes, and in different races, the form of the shaft at the middle was investigated and classified in six more or less evident groups. A series of casts of these types must prove to be of great value to instructors in somatology; it is to be hoped that Dr. Hrdlička will add to the comparative value of the series by a further study of the tibia of the anthropoid apes.

In the November number of the same journal appears an abstract of a paper read before the American Association for the Advancement of Science upon the "Physical Differences between White and Colored Children." The paper is too much condensed for the reviewer to make a satisfactory abstract. However, the general con-

clusions are that the "white children present more diversity, negro children more uniformity, in all their normal physical characters."

The *American Antiquarian* for the year 1899 will contain a list of accessions to, and of the specimens available for exchange in, the anthropological museums of America. This novel feature commends itself to the attention of curators. The appearance of *Antiquarian* would be greatly improved by more careful proof reading.

FRANK RUSSELL.

ZOOLOGY.

Dimorphism in Crepidula. — An interesting instance of environmental and sexual dimorphism is described by Prof. E. G. Conklin [1] in the sedentary gasteropod Crepidula. In *C. convexa* there are marked local varieties depending upon the immediate environment; for example, when found upon the shells of Illyonassa or Littorina it is deeply convex and darkly pigmented, but on oyster shells it is very much flatter and lighter in color. Also among the larger species, *C. fornicata* and *plana*, marked differences in the form of the shell occur, which are due to the nature of the substratum to which they are attached. These irregularities in form are not inherited and are cited as examples of "environmental polymorphism." Dimorphism in *Crepidula plana* is exhibited by the occurrence of a dwarf form in addition to the normal one. The latter is found inside of dead shells of Neverita inhabited by the large hermit crab *Eupagurus bernhardus*, while the dwarf is found in the smaller shells of Illyonassa and Littorina, inhabited by the little hermit *Eupagurus longicarpus*. The disproportion between the normal and dwarf forms is considerable, the former being about thirteen times as large as the latter, as was determined by measurement of body volumes. Age and sexual maturity are attained in the dwarf as in the normal type. All of the organs of the body are reduced in size in about the same proportion, but the cell size in homologous organs of the two forms, including the ova and embryos, is constant. There is thus a smaller number of cells present in the various organs and also in the entire body of the dwarf than in the giant. The dwarfed form is correlated with the small size of the shell in which

[1] Conklin, E. G. Environmental and Sexual Dimorphism in Crepidula, *Proc. Acad. Nat. Sci.*, Philadelphia (1898), pp. 435-444, Pls. XXI-XXIII.

it has found lodgment, but some other factor — as yet undiscovered — than diminished supply of food and of oxygen, reduction of locomotion, or limitation of growth by pressure, is involved in its production. Whatever the cause, it operates by stopping cell growth and division. The dwarfs are not a permanent race, but are constantly recruited from the young of the giants. A few specimens have been found whose shell structure indicates the growth of a typical form from the dwarf condition in consequence of a change to a more roomy home. The dwarfs are what they are by reason of external conditions and not of inheritance. They present a "physiological variety" in which the shape and size of the body, as well as the number of cells in the entire organism, are modified by the direct action of the environment. There is no evidence that these modifications have become heritable. Sexual dimorphism is also well marked in *C. plana*, the average female being about fifteen times as large as the average male. As in the case of the dwarfs, the smaller size is due to the smaller number of cells in the body. Measurements of individual cells of the intestine, stomach, liver, kidney, muscles, and epithelium show that cell size remains the same in the male and female. Whatever the ultimate cause of the smaller size of the male, it operates, as in the dwarf, by causing a cessation of cell growth and division.

C. A. K.

British Entomostraca. — The natural history of the fresh-water Entomostraca of Epping Forest, a woodland tract of 35,000 acres, is given by Mr. D. J. Scourfield,[1] from observations extending through a number of years. The author records 102 species, the most complete local list hitherto published for the British Isles, from which a total of 190 species has been reported. One American form, *Ceriodaphnia scitula* Herrick, and several continental species are reported for the first time from this region. The cosmopolitan distribution of this group is further emphasized by the fact that, with one or two doubtful exceptions, all British species occur in continental Europe, and most of them have been found in North America, and not a few in South America, South Africa, Australia, and New Zealand. On the other hand, districts of limited extent, with characteristic physical features, ofttimes exhibit great differences in their Entomostracan fauna. Hydrological rather than climatic factors

[1] Scourfield, D. J. The Entomostraca of Epping Forest, with some general remarks on the group, *The Essex Naturalist*, vol. x (1898), pp. 193–210, 259–274, 313–334.

determine to a large extent the distribution of this group. The local and seasonal occurrence of each species is given in tabulated form. The Cladocera reach their maximum development, as regards species, in September, and their minimum in January. The Ostracoda exhibit two maxima in March and in September, the latter being more marked, and two minima in August and in January. The Copepoda exhibit but a slight variation in the number of species during the year, though there is some suggestion of maxima and minima similar to those detected among the Ostracoda. The seasonal distribution of males and ephippial females of the Cladocera is given for 34 species. In this group, considered as a whole, there are two seasons of sexual activity, the first reaching a maximum in May and affecting only 10 species, and the second extending throughout the autumn months, culminating in October, and affecting 30 species. Thus in the majority of the species the period of sexual activity is confined to the autumn months, though a small number is affected during both seasons, and a few exhibit but a single annual period in the spring. In only a single species, *Daphnia longispina*, does sexual reproduction continue from May to October. Colonies of a given species found in different aquatic habitats may present marked differences in sexual activity, the size of the body of water seeming to be correlated with the variations. Thus males and ephippial females of *Simocephalus vetulus* have been found in the spring only, in tiny pools and ditches, and again in the fall in bodies of water of slightly larger size, while no trace of either sexual form was observed in a larger pond examined repeatedly during a period of three years. The author is inclined to attribute these differences to the direct action of the environment.

A second paper[1] by the same author deals with the biology of a common water-flea in an interesting way. A respiratory function is assigned to the anal cæcum, a thin-walled triangular sac, with glandular cells in the dorsal wall. It is constantly dilating and contracting, and produces a circulation of water that suggests its respiratory function. As in the Cladocera generally, parthenogenesis prevails with alternating periods of sexual activity. The sexually mature female produces the so-called winter or resting eggs, which, unlike the parthenogenetic or summer eggs, require fertilization in order to develop. The ephippium, which carries the resting egg, is formed in Chydorus from the cast-off shell, which is somewhat thickened,

[1] Scourfield, D. J. Chydorus sphæricus, *The Annual of Microscopy* (1898), pp. 62–67, 1 plate.

especially along the posterior margin. The author suggests the name proto-ephippium for this primitive protective covering of the resting egg. This structure is formed quite independently of fertilization, but before the resting eggs leave the ovaries and are transferred to the ephippium it is necessary that they be fertilized. In the absence of the male the empty ephippium is cast off and resting egg retained. If fertilization does not ensue, this process may be repeated several times in succession, as Weismann has already shown for the larger Cladocera. Some of the secondary sexual characters of the male, as, for example, the form of the intromittent post-abdomen, are not assumed until the last molt preceding the adult condition, the structure preceding this molt being of the female type. Attention is called to the cosmopolitan distribution of the species, and to its preference for small bodies of water rich in vegetation. It is, however, not infrequently found in our largest American lakes. Like some other Cladocera, Chydorus exhibit two periods of sexual activity in each year, one in April and May, the other in November and December; the former is the more important, and affects only those individuals found in small ponds likely to be dried up during the summer, while the latter is confined to colonies in larger bodies of water. It seems probable that some colonies may never have a sexual period at all; at least some large ponds most thoroughly examined never once yielded a male or an ephippial female.

C. A. K.

Rotifers of the Leman. — The first part of a superbly illustrated monograph of the rotiferan fauna of this Swiss lake and its neighborhood has been published by Dr. Weber.[1] Owing to the absence of swamps and small bodies of water in this alpine environment the number of species recorded is not so great as in England or in Germany, though a very extensive list is presented. Each species is briefly described, the synonymy and the bibliography are given, and figures, often in natural colors, are liberally provided. The males and resting eggs are illustrated in some instances. We regret the absence of references to several important American lists in the bibliographies of the various species. American workers on this group will, however, find the paper of much interest, as many of the species figured are abundant in this country.

C. A. K.

[1] Weber, E. F. Faune rotatorienne du basin du Léman, *Revue Suisse de Zool.*, tome v (1898), pp. 263–354, Pls. X–XV.

Classification of the Centropagidæ.[1] — In two recent papers Mr. F. W. Schacht has given us a revision of the North American species of this family of Copepoda. On account of the widespread interest in this group among students of organisms in potable water, and of the food of fresh-water fishes, we reproduce here in tabular form the criteria for distinguishing the family and also its constituent genera.

a_1. Division of the body into cephalothorax and abdomen between the thoracic segment bearing the fifth pair of feet and the segment bearing the genital apertures. GYMNOPLEA.

b_1. In male: Anterior antennæ symmetrical or nearly so; and its secondary sexual characters not confined to peculiarities in structure of trunk, the antennæ, the fifth pair of feet, and the abdomen. Marine. AMPHASKANDRIA.

b_2. In male: Anterior antennæ unsymmetrical and secondary sexual characters generally confined to points enumerated above in (b_1). HETERARTHRANDRIA.

c_1. Rostrum present.

d_1. In female abdomen 3 or fewer segments; antennæ with 24 or fewer joints. Male with inner ramus of fifth feet rarely rudimentary. Marine. Family *Pontellidæ*.

d_2. In female abdomen never fewer than 3 segments; antennæ never fewer than 24 segments. In male, fifth pair feet are grasping organs. Family *Centropagidæ*.

c_2. Rostrum wanting. Family *Candacidæ*.

a_2. Division of the body into anterior and posterior parts in front of last (fifth) thoracic segment. PODOPLEA.

Subdivisions of the family Centropagidæ.

a_1. Thorax 6-jointed. Subfamily CENTROPAGINA.

a_2. Thorax 5-jointed.

b_1. Fifth pair feet degenerate; inner ramus, 0–1 jointed; outer ramus, 1–3 jointed. Abdomen of ♀, 3-jointed. Subfamily TEMORINA.

[1] Schacht, F. W. The North American Species of Diaptomus, *Bull. Ill. State Lab. Nat. Hist.*, vol. v (1897), Art. 3, pp. 97–208, Pls. XXI–XXXV. The North American Centropagidæ belonging to the Genera Osphranticum, Limnocalanus, and Epischura, *Bull. Ill. State Lab. Nat. Hist.*, vol. v (1898), Art. 4, pp. 225–269.

c_1. Furca with 3 large terminal setæ to each ramus.
Genus *Epischura*.

c_2. Furca with 4 large terminal setæ to each ramus; Inner ramus, legs 2–4, 3-jointed.

d_1. Inner ramus first pair legs, 2-jointed.
Genus *Diaptomus*.

d_2. Inner ramus first pair legs, 3-jointed.

e_1. Fifth pair legs of ♀ with *spines* on inner side of last joint of outer ramus. In male, outer ramus of fifth leg, 2-jointed on left side; 3-jointed on right side. Genus *Osphranticum*.

e_2. Fifth pair of legs of ♀ with long, plumose hairs on inner side of last joint of outer ramus. In male, both outer rami of fifth pair of legs 2-jointed. Genus *Limnocalanus*.

b_2. In female, abdomen 4-jointed and fifth pair of feet with 3-jointed outer ramus and 2–3 jointed inner ramus. In male, fifth pair of feet subchelate.
Subfamily Leuckartiina.

b_3. In female, abdomen 3- or 4-jointed. The fifth pair of legs has a 3-jointed outer ramus and a 1–3 jointed inner ramus; chetæ undeveloped or wanting in male.
Subfamily Heterochaetina.

A New Rhizopod Parasite in Man.—Professor Ijima[1] has described a new species of Rhizopod, *Amœbæ miurai*, parasitic on the human being. The patient in whom these parasites were found was a young woman suffering from abdominal tumors and an ascites-like accumulation of fluid in the abdominal cavity. She finally died of peritonitis and pleuritis endotheliomatosa. The amœbæ were found in abundance in the fluids of the peritoneal and pleural cavities as well as in the hemorrhagic discharges from the intestine. As many of the amœbæ in the abdominal fluid were dead, and as they died rapidly when kept in this fluid at the temperature of the body, it was suspected that the abdominal fluid was not a natural habitat. The living organisms were spherical or ellipsoidal in form, with a diameter from 15μ to 38μ, and had at one pole a small villous knob. Specimens

[1] Ijima, I. On a New Rhizopod Parasite of Man, *Annotationes Zoologicæ Japonenses*, vol. ii (October, 1898), p. 86.

with two or three nuclei were as frequent in occurrence as those with only one nucleus. Most specimens exhibited two or more large vacuoles. Their movements were at most sluggish.

G. H. P.

American Fishes. — The second part of Jordan and Evermann's comprehensive work on the *Fishes of North and Middle America* is just issued (Oct. 3, 1898) from the Government Printing Office. This volume of 943 pages contains detailed descriptions of 1883 species of fishes. The Sparoid, Sciænoid, Labroid, and Cottoid families and their allies make up the bulk of the present volume.

The third part of the catalogue of *The Fishes of North and Middle America* appeared about November 25. This concludes the text of the work, the remaining fourth part being devoted to plates and to a recapitulatory Check List. The three volumes, which are continuously paged, contain 3136 pages; 3127 species are described in detail, these being arranged in 1077 genera. The large number of genera recognized is in accord with the views of Dr. Gill and Dr. Bleeker, which other ichthyologists were slow in accepting. There is no doubt, however, that the convenience of the systematic zoologist is best met by the recognition of every tangible and constant structural difference as having value for generic distinction.

The four volumes constitute *Bulletin 47 of the United States National Museum.*

D. S. J.

Fauna and Flora of the Catskill Mountains.[1] — This paper contains a list of 58 trees and shrubs collected in the valleys of Schoharie Creek and Upper Katerskill and the surrounding mountains; 9 gastropods and a Sphærium, mostly from the Creek; Cambanes bartoni, 8 fishes, 8 batrachians, 2 snakes, and a fairly complete list of the mammals with copious notes.

Mr. W. P. Pycraft will study the megapodes collected by the Willey Expedition. Materials for the embryology were obtained.

The article by Ameghino upon an existing species of Mylodon (*Neomylodon listai*) is reproduced in *Natural Science* for November. The evidence is a piece of skin from Patagonia, the outer surface of which presents a continuous, not scaly, epidermis, covered with stiff, reddish hair about two inches in length. In the deeper layer of the

[1] Mears, L. A. Notes on the Mammals of the Catskill Mountains, New York, with General Remarks on the Fauna and Flora of the Region, *Proc. U. S. Nat. Mus.*, vol. xxi, pp. 341–360.

skin are dermal ossicles similar to but somewhat smaller than those of the extinct Mylodons. The only white person to see the animal alive was the traveler, the late Ramon Lista, who described it as about the same shape and size as the Indian pangolin.

Professor Poteat has recently shown that Leidy's genus Ouramœba is really Amœba plus mycelial hyphæ.

BOTANY.

California Plants in their Homes.[1] — This little book of Mrs. Davidson has more successfully brought together the choicest spirit of the ecology and physiology of plants than any other which has come the way of the writer of this notice. Being made for children, it has from this very fact all the more to teach the adult devotee of the science. In some sixteen chapters we are introduced to many kinds of plants and come away with more than a bowing acquaintance. We are introduced to many of the innermost mysteries of their lives and learn their innocent and contriving ways of obtaining for themselves the coveted advantages over their neighbors. While the book is written so that it may be used as a reader, the "supplement for the use of teachers," together with the style of the writing, make it one of the most possible of laboratory books for the younger student; in fact, the temptation to follow out the suggestions of the author to do this and to do that is so fascinating and so easy that it needs only the chance, supplied by the teacher, to be done. The book is, unfortunately, Californian only, but the lesson to teachers is as wide as the subject taught. Our text-books run now to physiology and ecology, but simply as classifications of physiological and ecological facts, from the consideration of which the poor student comes away knowing well that certain things are so, but with little notion of the reason why the teacher has been to such extraordinary pains to make him aware of it. But Mrs. Davidson is a teacher, — a teacher of teachers in fact, — and has realized the folly of thinking that we really get away from the evils of the old systematic teaching in botany by changing simply the subject-matter without changing the method and point of view.

While confessedly an elementary book, this is to be compared

[1] Davidson, Alice Merritt. *A Botanical Reader for Children.* Los Angeles, B. R. Baumgardt & Co.

with many of the books which have been issued for students of a higher grade than those technically for children. The facts brought forward are those usually taught only in books for the higher grades, but they are presented in such a way as to be equally intelligible to "children," with the farther advantage that the significance of the detail is never lost sight of — an advantage inestimable in its results. In the headings to the chapters, too, the author has displayed great ingenuity, and the results are not open to such criticism as may be directed toward those of some popular books and articles on botanical subjects.

The most unsatisfactory features of the book are the illustrations. They are too coarse to convey the idea desired in most cases, and the grace and delicacy of most of the plants figured have been lost entirely.

We feel that the teacher in California who attempts to realize from "nature study" in the lower grades that which is hoped for from it, will find a way pointed out by this book which is both clear and certain, and that the teachers in other states will realize their need of a similar book and find much assistance by using it and adapting it to their local needs.

W. A. SETCHELL.

Van Tieghem's Eléments de Botanique.[1] — While the first volume of this text-book, as would be expected from the author's many important contributions to vegetable anatomy and morphology, presents a clear and sound statement of this part of botany, the second volume, in the present edition, merits special mention as being the first readily accessible synopsis of the vegetable kingdom in which the dicotyledonous flowering plants are classified according to their seed and ovule structure along the line laid down by Professor Van Tieghem in a series of articles published some two years ago.[2]

Excluding the Nymphæaceæ, which, with the Gramineæ, he places in a class considered as being exactly intermediate between the Monocotyledons and Dicotyledons, the author divides the latter into two subclasses, Insemineæ and Semineæ, the first destitute of detachable seeds at maturity of the fruit, and the second bearing seeds. The first of these is then divided into groups marked by the presence or absence of transient ovules (which, when present, are

[1] Van Tieghem, Ph. *Eléments de Botanique.* Troisième édition. Paris, Masson et Cie., 1898. 2 vols., 12 mo. i. Botanique générale. ii. Botanique spéciale.

[2] Van Tieghem. Sur les Phanérogames sans graines, formant le groupe des Inséminées. *Comptes Rend.* **124**: 22 mars–3 mai, 1897.

homologized with carpellary leaflets, the embryo sac otherwise originating in the tissues of the simple carpel), and both subclasses are further subdivided according to the number and character of their ovule coats, while the ultimate grouping of families, though to a certain extent dictated by grosser characters, is influenced largely by the results reached in late years by the students of systematic plant anatomy.

As might be expected, the sequence of families is greatly modified from that which represents the conclusions of the English and German schools, though of the two it naturally conforms more closely to the latter, by which a more consistent effort has been made to represent phylogenetic affinities as indicated by anatomical as well as more obvious structure.

Whatever may be the general verdict on the new basis of primary classification and its present exemplification, — and it is likely to find more opponents than supporters, — the author is to be congratulated on having presented his views in a suggestive and convenient form for the guidance of future investigators; and the attempts which are sure to be made both to strengthen and overthrow it by the histological taxonomists can but result in laying further foundation stones for a truly natural system of the flowering plants. T.

Are Bacteria Fungi? — In *Centralblatt f. Bakteriologie*, etc., 2te. Abt., Bd. iii, Nos. 11 and 12, Dr. Johan-Olsen argues that bacteria are simply one stage in the development of fungi and supports his text, Zur Pleomorphismusfrage, with two well-drawn plates. Unfortunately, some of his most striking examples are drawn from species of Oospora which mycologists for many years have classed as fungi, and whose only claim to be classed as bacteria is the fact that when their extremely tenuous hyphæ break up into conidia, or oidia, the latter closely resemble rod-shaped bacteria in size and form. These conidia, however, grow into genuine branched mycelia. Some of the other cases which he cites, *e.g.*, branched tubercle and diphtheria bacilli, may well be involution forms, as Dr. Migula has suggested, since they are usually found only in old cultures, sparingly, and under conditions unfavorable to the organism. More difficult to explain is his account of the change of the mycelium of *Dematium casei* into bacteria bearing endospores, the germination of which spores he succeeded in witnessing. Possibly Dr. Ol. Johan-Olsen was working with mixed cultures. Much is said of Dr. Brefeld's *System*, but if Dr. Johan-Olsen's culture methods are not a very decided improve-

ment on those of his master, which have been described to me in recent years by a number of people who have studied at Münster, and which are certainly very crude, then we are fully warranted in calling in question the results. One is the more inclined to do this because in another paragraph we are told that: "Almost all bacteria, which I have had in cultivation in recent years, form a branched mycelium in course of time, especially all bacilli." We are also rendered suspicious by the statement that species of Aspergillus and Mucor may appear in the form of amœba. It is possible, of course, that bacteria are only "incompletely known fungi," but up to this time the evidence is certainly not very conclusive, and to the writer it seems not at all improbable that they may have had quite a different origin — at least many of them.

ERWIN F. SMITH.

Dr. Bolander. — In *Erythea* for October, 1898, Willis L. Jepson writes interestingly of Dr. Henry N. Bolander, botanical explorer, who died in Portland, Oregon, Aug. 28, 1897. He was born in Germany, but most of his life was spent in Ohio and on the Pacific coast. He was educated for a clergyman, but through the influence of Leo Lesquereux his energies were turned into scientific channels. The article is accompanied by a half-tone picture of the botanist. Thirty-seven species of flowering plants were named after Dr. Bolander.

E. F. S.

The Costa Rica Flora. — The second volume of the *Primitiæ Floræ Costaricensis*, begun by Pittier and Durand, is continued by the first-named author alone. But Part I of this volume, concerned with the Polypetalæ, is from the pen of Capt. John Donnell Smith, whose work on the flora of Guatemala is everywhere well known. As might be expected, several species are here described for the first time. Descriptions of a number of species previously published in the *Botanical Gazette* are reprinted, for obvious reasons.

Urban's West Indian Flora.[1] — The first fascicle of this work, with which Dr. Urban has been known to be occupied for some years past, reaches page 192, and is entirely devoted to a botanical bibliography of the West Indies. The botanical treatment itself is awaited with much eagerness.

T.

[1] Urban, I. Symbolæ Antillanæ seu fundamenta floræ Indiæ occidentalis, 1. 1. Berolini, Fratres Borntraeger, 1898.

Bailey's Evolution of our Native Fruits.[1] — Though Professor Bailey is a horticulturist and commonly writes for horticulturists, he is well known to botanists as an accomplished botanist. To say that the nine chapters comprised in the present book are devoted to the rise of the American grape, the strange history of the mulberries, the evolution of American plums and cherries, the native apples, the origin of American raspberry-growing, evolution of blackberry and dewberry culture, various types of berry-like fruits, various types of tree fruits, and general remarks on the improvement of our native fruits, tells little of the wealth of detail that it contains. Group after group is monographed, and people in search of disentangled snarls of nomenclatural detail need seek little further than the present work for models of conservative upheaval when upheaval becomes necessary. As to the horticultural side of the book, little need be said: it was written for horticulturists.

T.

Poisonous Grains. — It has long been believed that the fruit of *Lolium temulentum* is poisonous, and chemists have had something to say about its toxic principles. In the *Journal de Botanique* for August, M. Guérin publishes an article embodying the results of a study made at the École supérieure de pharmacie of Paris, in which he records the constant occurrence of fungal hyphæ in the nucellus of the ovule and the layer of the caryopsis lying between the aleurone layer and the hyaline portion of the wall. These hyphæ, which appear not to have been identified with any fruiting form, are referred to as, perhaps, the cause of the toxicity of the Loliums in which they occur (*L. temulentum*, *L. arvense*, and *L. linicola*), and they are stated not to have been found in *L. Italicum*, and only once in *L. perenne*. The fungus is compared with *Endoconidium temulentum*, Pril. & Delacr., found in diseased grain of the rye, and believed to be the cause of some of the cases of poisoning attributed to that grain, though it is believed to differ from the fungus named, and the conclusion is reached that, unlike this species and Claviceps, it lives in the maturing grain symbiotically rather than as a parasite.

Botanical Notes. — The issue of *Möller's Deutsche Gärtner-Zeitung* of October 22 may be called a Clematis number. It is well illustrated and contains a number of articles on the cultivated forms of this attractive genus by well-known writers.

[1] Bailey, L. H. *Sketch of the Evolution of our Native Fruits.* New York, The Macmillan Company, 1898. 8vo, xiii + 472 pp., 125 ff.

Ranunculus Andersonii Gray is made the type of a new genus, Beckwithia, by Jepson, in the October number of *Erythea*. Unfortunately, the fact that the two are identical was not discovered soon enough to prevent the one species from appearing on the plate as *B. Andersonii*, and in the text as *B. Austinæ*.

Anton Pestalozzi's revision of the Capparidaceous genus Boscia, reprinted from the *Bulletin* of the Boissier Herbarium, forms No. 7 of the "Mittheilungen aus dem botanischen Museum der Universität Zürich."

The acaulescent violets of the eastern states are the subject of further observations by C. L. Pollard, in the *Botanical Gazette* for November. Twenty-seven species of this type are now distinguished in the key, but even then the author adds that he fully realizes "the futility of constructing any key in the hope that it will afford conclusive determinations of every unusual form." "Habit as well as habitat, the texture of the herbage, color of the flowers, position of the cleistogenes, nervation, shape and degrees of pubescence of the leaves, nature of the surrounding vegetation," are all taken into consideration in the separation of species for which herbarium material is said to be absolutely worthless unless one is fortified by previous familiarity with the growing plant.

Hydrophyllum tenuipes is the name given by Heller in the *Bulletin of the Torrey Club* for November to a plant from the state of Washington.

The hybrid between *Lobelia syphilitica* and *L. cardinalis*, which sometimes occurs spontaneously in this country, appears to have been produced in cultivation in France, and to have been again crossed on *L. cardinalis* (*Annales Soc. Bot. de Lyon.* **22**: 8).

Part III of Professor Comes's memoir on tobacco [1] is a classified account of the introduction, cultivation, and use of tobacco in Asia and Oceania.

M. C. de Candolle contributes a paper entitled "Piperaceæ Bolivianæ" to the *Bulletin of the Torrey Botanical Club* for November. Two new species of Piper and ten of Peperomia are described.

Actinidia Kolomicta, a climbing plant, the foliage of which is quite as brilliantly colored as that of the commonly cultivated and popular Acalyphas, is figured in color in *Die Gartenwelt* of November 6.

[1] Comes, O. Del tabacco. Storia, geografia, statistica, speciografia, agrologiæ patologia. III. Napoli, 1898. (*Atti R. Ist. d'Incorrag. di Napoli.*)

An interesting paper is that by Rowlee and Hastings in the *Botanical Gazette* for November, on the seeds and seedlings of some Amentiferæ.

Septal nectaries, quite common in several of the larger families of Monocotyledons, are now noted by Van Tieghem for *Cneorum tricoccum*, for which it is proposed to create the new combination *Chamælea pulverulenta* (Vent.). (*Bull. Muséum d'Hist. Nat. Paris*, **1898**: 241).

Luzula campestris and related species form the subject of a neat little brochure, with a good plate, reprinted by Buchenau from the *Oesterreichische Botanische Zeitschrift* of 1898.

The Cyperaceæ of British India have been tabulated by C. B. Clarke in the *Journal of the Linnæan Society, Botany*, No. 235, with reference to their geographical distribution. Eleven areas are recognized from this point of view. The paper is thus virtually an appendix to the *Flora of British India* of Sir Joseph Hooker.

Dr. Elliott Coues, in the *Proceedings of the Academy of Natural Sciences of Philadelphia*, 1898, Part II, publishes a critical article on the localities for the plants of Lewis and Clark's herbarium, a list of which, apparently rather inaccurate in some respects, was some time ago published by Mr. Meehan.

"A Few Notes on Canadian Plant-lore," by Carrie M. Derick, and "A Review of Canadian Botany from 1800 to 1895," by D. P. Penhallow, are the titles of Nos. 6 and 7 of the *Papers from the Department of Botany of McGill University*, reprinted, respectively, from the *Canadian Record of Science* and the *Transactions of the Royal Society of Canada.*

From its title, the *Queensland Agricultural Journal* would not be turned to by the systematic botanist, but its current issues contain a goodly number of descriptions of new species of Australian and Papuan plants by F. Manson Bailey, the colonial botanist of Queensland.

Science, of November 18, contains a preliminary paper on the fauna and flora about Coldspring Harbor, L. I., by Professor Davenport, and an article by Dr. Mead on an unusually abundant occurrence of a species of Peridinium in the waters of Narragansett Bay last summer, giving an intense red color and a very disagreeable odor to the water, and killing many fish and crustacea.

The last part of the *Wissenschaftliche Meeresuntersuchungen* of the Kommission zur wissenschaftlichen Untersuchung der deutschen Meere in Kiel is largely occupied by phycological plankton studies.

Heft 5 of *Hedwigia* for 1898 is occupied by the conclusion of C. Mueller's "Analecta bryographica Antillarum," and the first installment of Hennings's "Fungi Americani-boreales." Among the latter Hennings is still finding a good grist of new species.

Bolander, well known by name at least to all students of Californian botany, is the subject of a biographical sketch, with portrait, in *Erythea* for October.

In the *Proceedings of the Linnæan Society*, October, 1898, is given a half-tone figure of the special gold medal presented to Sir Joseph Hooker by the Society on the occasion of the completion of his *Flora of British India.* The obverse bears a relief bust of Dr. Hooker, modeled very faithfully by Bowcher, while the reverse is margined by a wreath of Sikkim rhododendrons, surrounding a suitable inscription.

The American Botanist is the name under which another journalistic effort is launched by Charles Russell Orcutt. While his previous papers have hailed from the Pacific coast, this, of which Vol. I, No. 1, appeared in September, seems to come from the Gray Herbarium of Harvard University, though a note by Dr. Robinson in the *Botanical Gazette* makes it appear that it is not to be regarded in any way as an official publication of the herbarium. The initial (and unique?) number is devoted to "an attempt at forming a record for the botanic garden of Harvard University, aiming to present the history and individuality of each specimen plant,"— a point in which Mr. Orcutt is believed to consider most American gardens very defective, — and deals with the cacti, not even excluding the glass models of the Ware Collection.

PALEONTOLOGY.

Habits of Thylacoleo. — In a recent number of the *Proceedings of the Linnæan Society of New South Wales*, Dr. R. Broom revives the question of the habits of a remarkable extinct Australian form, which led to a famous controversy between Sir Richard Owen and Sir William Flower. In 1859 Owen presented Thylacoleo as "one of the fellest and most destructive of predatory beasts, with affinities to the Dasyuridæ." Later, moreover, in 1866, he adhered to this interpretation of the large back cutting teeth, although in the mean time a pair of procumbent tusks had been discovered, which appar-

ently related this form to the herbivorous diprotodont marsupials. In 1868 Flower presented the entirely contradictory view, that Thylacoleo differed in every respect from the carnivorous marsupials, and was simply a harmless vegetable feeder, totally unfitted for preying upon the large contemporary marsupials. This called forth a violent reply from Owen in 1871. But subsequently Flower's position was supported by Krefft and Lyddeker, and is the one now generally received.

Dr. Broom's excuse for reviving this question is, that in general he has concluded to support Owen's opinion. He says there are insuperable difficulties in the way of considering Thylacoleo as a bulb or fruit eater. With its remarkable dentition such an animal would be unable to do more than slice its fruits and vegetables, even if it could have procured both in abundance. With succulent roots and bulbs the same difficulty arises as with the fruits; that even the most succulent, if we could suppose them digestible in slices, cannot be had in a succulent condition all the year round. When we look at Thylacoleo, he continues, we find not only the enormous temporal muscles and only moderate masseters, as in carnivorous animals, but that everything about the skull seems to be built on carnivorous lines. There is thus in his opinion no other conclusion tenable than that Thylacoleo was a purely carnivorous animal, and one which would be quite able to kill and probably did kill animals as large or larger than itself. He then proceeds to show in what manner Thylacoleo could have originated from small, shrew-like forms of Phalangers.

In the reviewer's opinion this revival of Owen's view is quite unjustifiable. It appears that the main argument of Dr. Broom is based upon the relations of the muscles of the jaw, and in reply it may be observed that all the early types of North American Herbivora of the Eocene period have enormous temporal fossæ and powerful sagittal crests *as an inheritance from their Unguiculate or clawed ancestors.* These temporal fossæ are so different from the rounded skulls of recent Herbivora, that one is very apt to be misled. The reviewer recalls very distinctly his discovery twenty years ago of the back portion of the skull of Palæosyops, an ancestral Titanothere, with its powerful zygomatic arches and large sagittal crest. These carnivorous structures led to the entirely mistaken belief that this peaceful herbivore was a new and exceptionally large carnivore. Dr. Broom's reasoning appears to be entirely similar and equally false.

H. F. O.

PETROGRAPHY.

Marble. — Vogt[1] has given an abstract of the geology of marble deposits, together with an account of the structure and mechanical properties of the rock. He defines marbles as metamorphosed limestones in which complete crystallization has occurred, and divides them into regionally metamorphosed marbles and those produced by contact action. The latter are characterized by the presence of garnets, vesuvianite, scapolite, wollastonite, etc., and the former by the presence more particularly of quartz, grammatite, actinolite, and other hornblendes. Nearly all the marbles of commerce are dynamically metamorphosed rocks. The difference in microscopic structure between the two classes of marbles is illustrated by several figures representing thin sections, and the difference between calcite and dolomite marbles is illustrated by several other figures. Grains of dolomite are shown to interlock by much less complicated contours than those of calcite. The reasons for their variations in structure are discussed at some little length. The article is a thorough one in every respect and is well worth study.

Grits Metamorphosed into Crystalline Schists. — Callaway[2] describes the transformation of a series of grits and shales into what he regards as true crystalline schists. The phenomena were observed near Amlwch, North Anglesey. Grits, which in their original form are clearly clastic, have been changed by dynamic agencies into chlorite schists. The quartz grains of the original rocks have been changed into areas of interlocking grains, and the matrix in which they were imbedded has been altered to a felt of chlorite, of mica, or a mixture of the two. In extreme cases the quartzes have been squeezed out into lenticules and bands of quartz mosaic, and between these have developed bands of chlorite and muscovite.

Dioritic Rocks of the Pusterthal. — Spechtenhauser[3] and Cathrein[4] give us very thorough accounts of the dioritic dikes and stocks at St. Lorenzen in the Pusterthal. The former describes the dike forms as diorite-porphyrites and norite-porphyrites. The diorite-porphyrites include quartz-mica-porphyrites, quartz-hornblende-por-

[1] *Zeits. f. prakt. Geol.* (1898), pp. 4 and 43.
[2] *Quart. Journ. Geol. Soc.*, vol. liv (1898), p. 374.
[3] *Zeits. d. deutsch. geol. Ges.*, vol. l (1898), p. 1.
[4] *Ibid.*, p. 257.

phyrites, and augite-diorite-porphyrites (kersantites). The norite-porphyrites are all quartzose. Besides these he gives a few notes on some granular stock-rocks that are intermediate in composition between quartz-diorites and quartz-norites.

Cathrein points out the fact that the porphyrites have a granular groundmass and in other respects are closely allied to granular diorites. Among these he mentions the existence of töllites, vintlites, and suldenites. The töllites differ from the tonalite-porphyrites in being more basic and in containing a very little quartz but a large quantity of garnet. The vintlites contain dihexhedra of quartz as phenocrysts in a fine-grained green matrix. The type is not that described by Rosenbusch in his "Physiographie." The author would include all the rocks above described and those of Klausen under the name "Klausenite." They vary in composition between biotite-hornblende-diorites and corresponding rocks in which orthorhombic and monoclinic pyroxenes and often some quartz occur. The variation in their structure appears to be due to their varying composition rather than to their mode of occurrence. From the fact that diorites, norites, and gabbros are often found to intergrade, he regards them as constituting a great family. The Klausenites are the quartziferous forms of these. The author concludes his discussion with an argument against the use of different names to designate the dike and effusive forms of the porphyrites. He would class them together as diorite, norite, and gabbro-porphyrites.

Three California Rocks. — A peculiar dike rock cutting the granodiorite on the ridge between Butte and Plumas Counties, California, consists of quartz, plagioclase, and needles of an amphibole in a granitic aggregate. The amphibole is in largest quantity. Turner[1] reports its composition as follows:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	NiO	CaO	BaO	MgO	K_2O	Na_2O	$H_2O<110°$	$H_2O>110°$	Total
54.64	.61	12.09	1.81	5.03	.13	.05	7.74	.05	11.86	1.01	2.35	.12	2.44	= 100.01

A new amphibole-pyroxene rock is also described by the same author from Mariposa County, California, and a quartz-alunite rock from Indian Gulch in the same county. The former is made up of augite and amphibole grains, a little quartz, and some pyrrhotite, forming a matrix through which are scattered large phenocrysts of brown amphibole. The quartz-alunite rock is a metamorphosed clastic. An analysis of the alunite separated from it gave:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	CaO	K_2O	Na_2O	H_2O at 100°+	SO_3	Total
2.64	.40	38.05	.23	.55	4.48	2.78	11.92	38.50	= 99.55

[1] *Amer. Journ. Sci.*, vol. v (1898), p. 421.

An Acid Pegmatite in a Basic Rock. — An interesting occurrence of an acid pegmatite in a basic rock is described by Jaggar.[1] It exists as lenticular or vein-like masses that merge gradually into the diabase forming the Medford dike in the Boston basin. As the pegmatite, which is a quartz-microcline aggregate, is approached, the white plagioclases of the diabase acquire a salmon-colored zone of more acid material. The plagioclase finally disappears and microcline takes its place. At the same time quartz replaces all the bisilicates. In some places the pegmatite appears to have been infiltrated into miarolitic spaces. Quartz inclusions in the diabase are often surrounded by inner zones of micropegmatite and outer ones of augite. The latter are the usual reaction rims so frequently discovered around acid inclusions in basic magmas. Within these the liquids bearing the pegmatite-producing minerals deposited their burdens and at the same time corroded the quartz of the nucleus. The conclusion arrived at, to the effect that granophyric intergrowths of quartz and feldspar are not necessarily primary growths, seems to be well substantiated.

Notes. — Vogt[2] gives a *résumé* of the facts bearing on the theory of deposition of ore bodies by differentiation processes in eruptive magmas. He points out that in the same magma we often find oxidation products, sulphides and metals, all of which must be regarded as normal differentiation products of the rocks in which they occur. The facts of differentiation are well known, but of the cause of differentiation we are yet ignorant.

Three kersantite[3] dikes cut the kamm-granite north of the Leberthal near Markich in Elsass, and several sheets and dikes of quartz porphyry occur in the Robinatthal.

The crystalline rocks of southeastern New York, east of the Hudson, are granites, gneisses, mica-schists, serpentine and basic and acid intrusives. Merrill[4] reports the oldest of these rocks to be a hornblende granite. This forms the central mass of the Highlands. On its flanks are banded gneiss composed of orthoclase and quartz, biotite and hornblende and containing beds of magnetite; and associated with this is the well-known mica-schist of the district.

[1] *Amer. Geol.*, vol. xxi (1898), p. 203.

[2] *Compte-rendu du cong. géol. intern.* 6e Sér. (1894), p. 382.

[3] Bruhus, W. Mitth. d. geol. Landesanst. v. Elsass-Loth., vol. iv (1897), p. cxxix.

[4] Merrill, F. J. H. *New York State Museum Report* (1896), p. 21.

These rocks are intruded by redbiotite-granite, diorite, norite, serpentine and gray granite. The mica-schist is regarded as a metamorphosed sediment. Under the sediments, but above the archean granites and gneisses, is the gray banded Fordham gneiss composed of alternating quartzose and biotite bands and injected parallel to the banding by pegmatite or granite. This, it is thought, may be an Algonkian sediment. The Yonker's gneiss, which in an earlier article the writer concluded to be a metamorphosed sediment is now thought to be an intrusive. All the serpentines of the district are also believed to have been derived from igneous rocks.

SCIENTIFIC NEWS.

Dr. W. R. Ogilvy Grant, of the British Museum, and Mr. H. O. Forbes, of the Liverpool Museum, have gone on a collecting trip to the Island of Sokotra.

The University of Indiana will locate its biological station this year at Warsaw, Ind. Applications for accommodations should be addressed to Professor C. H. Eigenmann, at Bloomington, Ind.

Tufts College opened a temporary zoological station last summer on the shores of Casco Bay. The authorities now have under consideration the establishment of a permanent station in that locality, which is remarkably rich in animal life.

The University of Kasan is sending out an expedition to Central Asia under the direction of Professor Sorolin. Geology, geography, and ethnology will be the most prominent subjects of investigation.

Dr. Charles F. Millspurgh, of the Field Museum of Chicago, goes on his fourth expedition to Yucatan, where he will continue his studies of the flora of that country.

Mr. D. G. Fairchild, of the United States Department of Agriculture, has started on a second long journey around the world, his first objective point being South America.

We regret to announce the death of Mr. Gilbert H. Hicks, botanist in charge of the seed control work of the United States Department of Agriculture. Mr. Hicks died on December 5, after a brief illness, aged 37. His official position was that of First Assistant in the Division of Botany. The *Asa Gray Bulletin* was also under his editorial charge. He leaves much unfinished scientific work.

The Munich Academy of Sciences has elected Professor Barrois, of Lille (geologist), and Professor Hartig, of Munich (botanist), to membership.

Professor George T. Allmann, well known for his investigations upon the Fresh-water Polyzoa and upon the Gymnoblastic Hydroids, died in November. He was born in Ireland in 1812, was appointed professor of natural history in the University of Dublin in 1844. In 1855 he was called to a similar chair in the University of Edinburgh,

where he remained until 1870, when he was succeeded by the late Prof. Wyville Thompson.

Cornell University will maintain summer schools during the coming summer in botany, entomology, geology, and zoology.

Dr. G. K. Niemann, professor of geography and ethnology in the Indian Institute in Delft, Holland, has resigned after twenty-five years of service.

Professor F. W. C. Areschoug, the well-known botanist, has resigned from his chair in the University of Lund, Sweden.

The Natural History Society of St. Petersburg has established a biological station on the shores of Lake Bologoy.

Recent appointments: Dr. R. T. Anderson, instructor in histology and embryology in Harvard Medical School. — V. H. Blackman, fellow in botany in St. John's College, Cambridge. — Dr. G. Bodi, assistant in the botanical institute at Innsbruck, Austria. — Dr. G. P. Eaton, assistant in osteology in the Peabody Museum of Yale College. — Dr. Marcus S. Farr, curator of the zoological collections in the state museum at Albany, N. Y. — Stanley Flower, of Bangkok, superintendent of the Cairo Zoological Gardens. — Dr. A. Y. Grevillius, of Münster, assistant in botany in the agricultural experiment station at Kempen on the Rhine. — Professor Hofer, of Munich, professor of geography in the University of Würzburg. — Mr. F. G. Hopkins, lecturer in chemical physiology in the University of Cambridge. — Dr. K. Keller, professor of zoology in the University of Zürich. — John Alden Loring, assistant director of the New York Zoological Gardens. — Mr. Horace Middleton, fellow in zoology and physiology in Magdalen College, Cambridge. — E. A. Minchin, lecturer on biology in Gray's Hospital Medical School, London. — Herbert Osborn, of Ames, Iowa, professor of zoology in the University of Ohio at Columbus. — Wladimir I. Palladin, professor of botany in the newly established technical school in Warsaw. — Alexander Hamilton Phillips, assistant professor of mineralogy in Princeton University. — Dr. Mark V. Slingerland, of Cornell, state entomologist of New York. — Dr. F. R. Stubbs, instructor in histology in Harvard Medical School. — Dr. C. O. Townsend, botanist and plant pathologist of Maryland. — Mr. Swale Vincent, Sharpey physiological scholar and chief assistant in the physiological laboratory of University College, London. — Dr. Frederick A. Woods, instructor in histology and embryology in Harvard Medical School.

Recent deaths: Prof. Rudolf Adamy, director of the ethnological museum at Darmstadt, aged 48. — Dr. James Edward Tierney Aitchison, author of numerous papers on the flora of India, in Kew, England, September 30, aged 63. — Dr. C. J. Backman, the Swedish botanist, May 1, in Stockholm. — James Behrens, formerly an active student of the Lepidoptera, at San José, Cal., March 6, aged 74. — Pasquale Conti, botanist in Lugano. — Sir George Gray, well known for his investigations of the resources of Australia and New Zealand, Sept. 19, 1898, aged 86. — G. E. Grimes, assistant on the geological survey of India, April 11, aged 26. — James Hardy, of Cockburnspath, England, a student of the zoology of northern England, October, aged 84. — Max Hauer, microscopist and mineralogist in Oberhausen, Germany, August 10, aged 51. — Dr. W. Kochs, privat docent for physiology in the University of Bonn. — Dr. Luigi Lombardini, professor of the anatomy of domesticated animals in the University of Pisa, June 27, aged 67. — Dr. Karl Mettenheimer, formerly a student of the invertebrates, in Schwerin, Germany, September 18, aged 74. — Johnson Pettit, entomologist, at Grimsby, Canada, Feb. 18, 1898. — Dr. Alexandre Pélliet, curator of the anatomical collections of the Musée Dupuytren in Paris. — Edward Tatnall, a student of the flora of Delaware, at Wilmington, May 30, aged 79. — Dr. Giambattista Valenzia, zoologist, at Pantelleria, June 15.

PUBLICATIONS RECEIVED.

(The regular exchanges of the *American Naturalist* are not included.)

ECKSTEIN, KARL. Repetitorium der Zoologie. Ein Leitfaden für Studirende. Leipzig, W. Engelmann, 1898. viii + 435 pp., 281 figures. 8 Marks. — HERTWIG, R. Summaries in Systematic Zoology. Translated and adapted by Albert A. Wright. Second edition. Oberlin, O., E. F. Goodrich, 1898. 35 pp. — PEABODY, J. E. Laboratory Exercises in Anatomy and Physiology. New York, Henry Holt, 1898. x + 79 pp., 8vo. — VERWORN, MAX. Beiträge zur Physiologie des Centralnervensystems. Erster Theil. Die sogenannte Hypnose der Thiere. Jena, Gustav Fischer, 1898. vi + 92 pp., 18 figures. 2.50 Marks.

BESSEY, E. A. Comparative Morphology of the Pistils of the Ranunculaceæ, Alismaceæ, and Rosaceæ. *Bot. Gazette.* Vol. xxvi, No. 5. — SHERWOOD, W. L. The Frogs and Toads Found in the Vicinity of New York City. *Proc. Linn. Soc. of N.Y.* 1897–1898, No. 10.

Bulletin of the Johns Hopkins Hospital. Vol. ix, No. 92. November. — *Geographical Journal.* Vol. xii, No. 6. December. — *Indiana Academy of Science.* Proceedings. 1897. — *Maryland Agricultural Experiment Station.* Bulletin No. 57. Report on the San José Scale in Maryland, by W. G. Johnston. — *Revista Chilena de Historia Natural.* Anno II, No. 9. September. — *Revista de Sciencias Naturaes e Sociaes.* Vol. v, No. 20. Porto, 1898. — *Royal Society of Victoria.* Vol. xi (N. S.), Pt. i. — *Sociedad Cientifica "Antonio Alzate"* (Mexico), *Memorias y Revista.* Tome xi, Nos. 9–12. 1897–1898. — *R. Universita degli Studi di Siena, Bull. del Laboratorio ed Orto Botanico.* Anno I, Fasc. 2, 3, June.

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